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THE PROGRESS OF CHEMICAL THEORY: ITS HELPS AND HINDRANCES.

BY DR. PERSIFOR FRAZER, Prof. of Chemistry.

[*Introductory lecture to the chemical course delivered before the Franklin Institute, November 21, 1890.*]

It is the ordinary reproach to science of the ignorant and disingenuous, that its conclusions at any given period are no more stable than the wildest speculations of the fanatic and dreamer.

We read continually in the papers some arrant nonsense said to have been pronounced by "one of the eminent scientists" of such-and-such a place and time, which the course of events has disproved; and the public is left to the conclusion that the gains of science are only air castles certain to dissolve when they become unpopular, and certain to lose popularity when the first pleasurable effect of the announcement has passed away. The following extract, from the *St. Louis Republic*, will furnish a case in point:

"It has only been sixty years since a great mathematician demonstrated that a steamship could never cross the Atlantic because it would be impossible for her to carry enough fuel to last during the trip. Before he had hardly deduced his calculations a steamer from America glided into port."

The name of the eminent scientist is not mentioned, and it is safe to conclude that if any man made such an observation he either would have failed of recognition by the class to which he is said to have belonged, or he was false to the fundamental principles of inductive science. It is not the province of inductive science to establish what is impossible, but what is in various degrees likely. Its premises are facts and its conclusions are probabilities; in many cases weak, but in others so strong that they produce the same effect upon the mind as certainties. Nor is it true that the gains of science are evanescent. Parallel with the accumulation of observations run the generalizations upon them. These generalizations are usually passed through the purgatory of hypothesis before they attain the bliss of theory, but no theory is old enough yet to have become more than a theory, though some have stood so many tests of their truth as to carry the conviction of axioms.

In looking over the histories of the sciences one finds the same general course of progress. At the outset in the halcyon days of the old Greeks, it is likely that some wise words will be found to have been spoken concerning them all; words that astound us with the apparent insight they show into problems which it would seem that the last twenty-five centuries were needed to give. But the centuries were not all equally productive. There came across the path of every systematic study of the laws of nature, first the cultured blight of the Aristotelian philosophy, which assuming to know everything, in fact largely contented itself with verbal jugglery, whereas accumulation of facts was the only road to knowledge; obliterating the forward steps that had been made, and substituting in their place the evolution of the universe and its laws from within. It was a philosophy where the distinction between words and things was

obscured, and a natural fact was attained by means of a pretty syllogism.*

Vastly worse were the centuries which followed, known as the middle ages. Centuries of ignorance, selfishness, and crime; when the possession of any knowledge but that of an armorer was looked upon with distrust and ascribed to the devil.

The different natural sciences emerged from this barbaric condition one to three centuries ago, and under the liberty of enlightenment, with the stimulus of more general education; have attained an abode in the life of the race from which it is difficult to see how they can be displaced without such a general cateclysm as would nearly destroy the human race itself.

Amongst these sciences, that of chemistry has had such a marvellous career that it is, perhaps, the best example which could be selected of the progress just alluded to. It illustrates aptly not only the methods employed in building up an inductive science, but the things that have helped and those that have hindered a development which, nevertheless, in spite of all hindrances, must fill us with a sense of wonder.

Our reason for this is that from various causes the real growth of chemistry only began in the seventeenth century, and that even then it lost nearly a hundred years in the quagmires of a false hypothesis which not only directed the efforts of chemists into unfruitful fields, but destroyed the value of the conclusions they reached from their work. Yet, even with all these drawbacks, no domain of human investigation has been widened so rapidly and with such advantage to the world.

At the very outset of the subject we find a generalization of old Democritus (who lived 450 B.C.) so astounding in its character and so accurate in most of its statements that only in the past few years have chemists been able to reach these profound thoughts thrown across the ages into the

* It is intended to refer here to the exposition of the Aristotelian philosophy, by its disciples from about the time of the Christian era to the eighteenth century, and not to disparage the marvellous genius of Aristotle.

midst of the civilization of our time, as the legend has it that Bruce's heart was thrown by the Douglas into the hosts of the Saracens to stimulate the ardor of the Christian Knights to charge and recover it. But, unfortunately, no such effect was produced by the good old laughing philosopher; though at a snail's pace, and after a lapse of 2,300 years we have reached the spot. Briefly, as transmitted to Epicurus, and expanded by Lucretius B.C., 99–55, it was this.*

The universe consists of atoms and space. The atoms are of many forms and of different weights, and the number of atoms of each form infinite. Change is only the combination and separation of atoms! Atoms are in constant motion. "First beginnings" or atoms are never destroyed or worn out. The difference between a hard body like iron, and a soft body like air, is that in the first the atoms move to and fro within small distances; in the soft body they move freely or rebound from each other only at long intervals.

Bodies are partly "first beginnings," partly unions of "first beginnings." The properties of the bodies formed of the groupings of "first beginnings" need not be like the properties of the "first beginnings" themselves. "It matters much with what others and in what positions the first beginnings of things are held in union and what motions they do mutually impart and receive."

These views are extraordinary, and, with the exception of the difference in the form of atoms, which is a point beyond what we have been able to reach even now, the above contains a very fair statement of the atomic theory which is held by the most advanced chemists to-day.

How Democritus could have reached such conclusions is a mystery, but his annunciation of these recondite truths very well illustrates the fact that an hypothesis, be it never so beautiful and even true, if unaccompanied by facts to support it in no way helps the progress of natural science. Like every other guess it indicates merely the frame of

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* *Democrit-Aderitæ operum fragmenta.*

mind of the man making it. It is like a floating shadow on the sea of time. Perhaps it defines substance, perhaps only a cloud of fancy.

This seed thrown off by Democritus found no soil of facts on which to grow, from his time until late in the present century, although Gassendi, Canon and Provost at Digue, in France, after ages of ignorance, proposed it again, but without proof; and it is thought to have influenced the minds of Newton and Boyle.

This then is one example of an occurrence in the history of the science which to all appearance neither helped nor obstructed its progress unless in the indirect way of teaching men's minds to grasp large and comprehensive thoughts. All could not have been ignorance and degradation in Abdera (Thrace), or Miletus, or Athens, where a language existed capable of conveying from mind to mind thoughts like these, and where a mind was capable of conceiving such thoughts.

It teaches the student of natural history a lesson in addition to that of the old traveller's speculations, and it may serve to illustrate the difference which the late Prof. Clifford of Cambridge pointed out between accepting those conclusions of natural science which one has been taught, but has not personally investigated, and accepting what is said to have been revealed, but which, it is acknowledged, is not susceptible of any proof. In the one case the way is open to any one to pursue any single direction which has been before taken; measuring and judging of the correctness of the steps of one's predecessor; but in the other case there is no path anywhere, and the correctness of the position assumed cannot be judged. It is the difference between, on the one hand, handing the keys of a hundred trunks to a custom house inspector, who has at best time to examine but one or two, asking him to satisfy himself of the accuracy of your description; and, on the other, telling him that something indescribable ought to convince him more thoroughly of the contents of the trunks which he cannot inspect, than of those which he can. Speaking generally it may be said that

a proposition of which the steps which led to its acceptance cannot be indicated and followed, has no place at all in the domain of science, though it may be true.

Such propositions were those of Democritus above given and it is quite just that in the absence of logical proof they should have been excluded from the realm of science, and that to him who first showed reason for believing them should be accorded the honor of their discovery.

Of much less importance is the next hypothesis of the nature of things which we find annunciated by Aristotle in his quadrilateral of states: solid, fluid, dry (or warm), and moist (or cold), or what he supposed to be the elements of all bodies, viz: earth, air, fire and water. It was unfortunate, and yet in accordance with the usual march of events that this utterly inadequate and narrow guess should have fettered men's minds for 2,000 years, owing to the mighty hold which Aristotle took of all nations.*

As his historian remarks, Aristotle's works had a prodigious influence in Asia, and Europe, and Africa; among the Persians, Arabs and in Germany where part of his ethics were read in the churches on Sunday instead of the Bible. In the middle ages, too, these elements of Aristotle were imbued with a mysticism more than Platonian.

It was the spirit of that middle age when the ignorant classes being the powers, made patient scientific work difficult and dangerous, that learning was concealed under the mask of paradox and cryptogram as if it were a crime. Whatever Aristotle's view of his elements may have been, it took a new direction, beginning with Geber in the eighth century.

The first chemists were alchemists who sought the transmutation of base metals into gold; the philosopher's stone; and the elixir of life. These were represented by Geber (an Arabian alchemist of 760), Albert von Bollstädt (1193–1280), Roger Bacon (1214–1294), Raymond Lull (1235–1315), Arnald de Villanova, Caletonia (1235–1314), etc. Those who examined physical problems retained the Aristotelian view,

* See Aristotle, *A chapter from the history of science*, Lewes.

while the alchemists took more or less modified forms of Geber's doctrine, that the metals were composed of mercury and sulphur. As an instance of the confusion which reigned in the ideas of this time, some believed that these constituents of metals were real sulphur and real mercury, while others believed that qualities were intended by these terms. Geber ascribed to the sulphur the property of giving different colors to the metals.

At the end of the fifteenth century the alchemists had added salt to mercury and sulphur. Many regarded the Aristotelian elements as the ultimate; and mercury, sulphur and salt as the intermediate or proximate elements, as, for example, Basil Valentine, who extended the number of substances of which these were the ultimate elements, from metals to all known matter, but denied that they were the common substances which we know under their names.

In the early part of the sixteenth century the failure to find the philosopher's stone led to the decadence of alchemistical or transmutation chemistry, and the rise of iatrochemistry or that of healing. Paracelsus (1493–1541) taught that in a burning body the sulphur quality represented the inflammability, the mercury the sublimation, and the salt the ashes.

From this to the end of the seventeenth century disputes as to tenets were numerous, but no real progress was made, Agricola (1490–1555) attacked Paracelsus and fell back upon Aristotle. Libavius wrote the first treatise on chemistry (1595). Van Helmont (1577–1644) denied all Paracelsus' views and sought an universal solvent, which should be a panacea. He first recognized the existence of gases and quantitative relations, and opposed Aristotle's doctrines that fire was a body or earth an element: but believed water and air were such.

Glauber (1603–1668) though possessing variable views, invented better means for separating bodies. Sumert (1572–1637); Willis (1621–1675); Lemery (1645–1715) believed in five first principles—mercury (spirit), sulphur (oil), salt, water (phlegma) and earth. Lemery taught that these were in rapid motion, and thus gave rise to the obvious proper-

ties of things. He explained the well-known phenomenon of the calxes of the metals weighing more than the metals themselves, by supposing that in burning they absorbed fire material.

The real philosophy of chemistry commences with Robert Boyle (1622–1691), who denied the accuracy of the doctrines both of Aristotle, and the later alchemical and iatro-improvements upon them. He believed that heat had not the power to transform complex substances into their constituents, but on the contrary, sometimes produced complex out of simple substances, and sometimes was without effect. Other agencies than heat could produce the same effects. He strongly denied that one could predict the number of simple substances as Aristotle and his successors had done. He thought it probable, however, *that the so-called elements consisted of the same kind of matter*, differing only in the size, form, etc., of their respective smallest parts.*

It is well to pause for a moment here to consider these logical and scientific views of Boyle, not alone because they introduced for the first time a rational inductive system of chemistry, emancipated from the mysticism and superstition of the ancients, but also because they are typical of one of the greatest of helps to the progress of chemical theory, independent and fearless criticism.

Except the brilliant guess that the so-called elements consisted of the same kind of matter, Boyle's mission seems to have been to hew down the weeds and undergrowth which had impeded the march of the science; yet his services were invaluable, as without them no further progress could have been made. This fact illustrates also the injustice of the cry so popular in some cases when the fallacy of a proposition has been exposed:

"What have you to set up in its place?"

Surely it cannot be required of him who discovers a flaw in a supposed explanation that he should be always ready with a sound explanation. The two characters of mind,

* See Kopp's *Geschichte der Chemie.*

which are required to accomplish these very different tasks, are entirely unlike.

Plato and Aristotle probably regarded the lightning stroke as a natural phenomenon, and could have refuted the popular belief that it was the missile from Zeus' hand, but it required dozens of centuries of observation before even the most remote approach to an explanation of the phenomenon could be given.

As soon as the ground is cleared of rubbish, other and more rational theories have a chance to grow. Therefore, the iconoclast, if impelled by his sense of truth, and if considerate in his methods, is a necessary pioneer and axeman ahead of the great army of science. It is so much easier, however, to throw down than to build up, that the iconoclast business is often overdone by those who are incapable of any more skilled service to science, and who confound the art of attacking everything with the duty of overthrowing evil. All honor to Robert Boyle for calling a halt in the unbridled fancy of the chemists of his day and clearing the way for a new era! All honor to his deep insight into the workings of nature, that he announced independently what old Democritus had dimly foreshadowed 2,000 years before; and what it was reserved for a great chemist now living to put in words and carry almost to the state of an accepted theory: yet to neither of them will belong the credit of demonstrating the unity of matter, but to some one, it would seem, who shall pass the speculative stage and offer proof. It looks as if this were not to be long delayed.

Both by Boyle's destructions and by his conceptions he aided the progress of chemical theory, as few have done since his time, and chemistry or the study of the most intimate relations of matter, as distinguished from alchemy, magic or the healing art may fairly be said to have started with Robert Boyle.

Singularly enough the first sapling to spring up and occupy the new clearing made by Boyle was an error so gross that it seems to the youngest student of to-day grotesque in its clumsiness, and yet defended by some of the subtlest of sophists, it took 100 years to overthrow it.

And the most instructive part of its history is that it was finally overthrown by an argument which Boyle himself had employed; which had been employed by other sceptics, and explained away by the phlogistonists, and was ultimately and successfully refuted with the same experimental proof by a countryman of Boyle. It is often the case that an attack in front, over the very ground of numberless previous repulses is successful, and it was the case here as shall be briefly shown.

Stahl (1660–1734) was a physician of independent views, who adopted Becher's theory of combustion or the changeability of bodies by heat. He believed that he had settled experimentally this question:

"Is a common quality present in sulphur and carbon? or is one contained in the other?"

The generally accepted view at that time was diametrically opposite to that which Boyle held of combustion, and might be stated thus: Sulphur consists of oil of vitriol and some combustible body, which latter escapes in burning. Stahl combined oil of vitriol with an alkali and heating the combination with carbon obtained an alkaline sulphide similar to that produced by sulphur and an alkali. From this, sulphur (or vitriol) can be separated.

Therefore, the combustible in carbon and sulphur was the same!

Heating calxes of the metals with carbon, there resulted the metals. The metals were then composed of the calxes and this substance. Fats and oils produced the same effect with the calxes, and, hence, in them too was the same combustible substance.

Stahl called this combustible "Phlogiston."

This hypothesis was rapidly installed into the rights and dignity of a theory and rallied around it as such, some of the brightest and best minds for three generations.

It was not only faulty in its conclusions, but inadmissible in its steps, and should have incurred the opposition of every intelligent man who understood the limitations of inductive philosophy: but it occupied the vacant space left bare by the labors of Boyle, and with a

growth as luxuriant as it was pernicious to the attainment of truth, obstructed in many ways all valuable advance of chemical theory, while it lasted.

It may not be amiss before sketching its rise and overthrow, to point out here wherein its inherent fallacies should have condemned this hypothesis from the outset.

Hypothesis means a guess—a temporary structure erected by the employment of the imagination strictly governed by experience, for the purpose of more rapidly reaching a generalization than by waiting for all the facts which in the end will be necessary to sustain a full-fledged theory. After one or two facts bearing on a subject are ascertained, it often happens that the mind is directed towards the possible existence of a law which would explain them both, but numerous unknown and untried experiments must result in a certain way in order that this supposed explanation may stand.

With time and a constantly increasing experience more and more such facts are ascertained. If all fall into their places the hypothesis grows stronger and stronger in probability until by a large accummulation of such corroborations, the hypothesis passes the undefined line which separates it from theory, and becomes a theory.

This theory then goes on increasing in strength by each additional fact which is found conformable to it, until its convincing force is almost as great to the mind as one of the facts which are the bricks of its construction.

But if during this period of probation of an hypothesis or of a theory a single fact is well authenticated which is inconsistent with it, the hypothesis or theory must be abandoned. Of course, in the case of a theory which had been tried and proven hundreds of times and found to apply to newly-discovered facts, its abandonment would be held in abeyance until every effort had been made to prove the authenticity of the fact and its inconsistency with the supposed explanation; but if these were unalterably confirmed the hypothesis or theory must fall.

This constitutes the true principle of inductive philosophy, and only by pursuing this path rigorously can its processes lead to any good result.

The obvious difficulty of the phlogistic theory was that the calx, which was considered the simple body was heavier than the metal which was supposed to be the compound, and this circumstance was explained by supposing that the phlogiston which was absorbed from the fire when the calx was heated therein had the principle of "levity" as opposed to that of "gravity," and that the more of it which was collected in a body the lighter became that body.

Here was the unpardonable error of the phlogistonites in the defence of their theory.

[*To be continued.*]

RIVETED JOINTS IN BOILER SHELLS.

By William Barnet Le Van.

[*Read at the stated meeting of the institute, held November 19, 1890.*]

The high steam pressure which boilers are now carrying render it desirable that the utmost care should be taken to unite the various plates of which they are composed in such a manner that the joints shall not only possess great strength when constructed, but also that they shall be but little influenced by wear and tear.

A riveted joint is, in a certain sense, an imperfect part of a structure. It cannot be so designed as to be strained uniformly throughout. It has always certain surfaces materially weaker than the rest, at which, consequently, deterioration of material, or fracture, by the action of the load, is liable to occur. These surfaces of weakness are so related that the increase of area of the one involves a diminution of area of the other.

The joint, therefore, which will carry the greatest load before fracture will be that in which the stress reaches the breaking limit for each of those surfaces simultaneously. Since the rivet section can in general be increased only at the expense of the plate section in the strongest joint, the rivet and plate will reach their breaking strain simultaneously under the same load.

It would seem, therefore, that the proper proportions or respective areas of a riveted joint could be determined by

the ordinary rules of applied mechanics without the need of experiments. But that this is not so is probably due mainly to a second condition of imperfection in riveted joints. To apply the ordinary rules for the strength of materials to riveted joints it is necessary that the distribution of the stresses on the respective surfaces should be known.

If those stresses were as uniformly distributed, as in an ordinary bar test for tension or shearing, the problem would be simple. But, in fact, the stresses are less uniformly distributed, and the law of their distribution is unknown. Consequently, the average stress on the surface of fracture of a riveted joint, when broken by a load, is less than it would be if the stress were uniformly distributed, and needs to be determined by special experiments. Further, it may be different for different forms of joint. This average stress, always less than the maximum stress which causes fracture, is here termed the apparent breaking stress. Hence, the chief object of experiment on riveted joints is to determine the apparent breaking stresses.

(1) For the different surfaces at which each joint may fracture.

(2) For the different forms of joints.

In certain cases allowances may have to be made for progressive deterioration of a joint, by corrosion or otherwise, which reduces the strength in certain directions more than in others. No experiments showing the amount of deterioration in such cases appear to have been made.

It is well known that the greatest strain upon plates of cylindrical boilers comes upon the horizontal joints, the pressure tending to burst the shell in the radial direction. The pressure of the steam upon the heads and the strain upon the transverse seams or joints is but one-half that tending to rupture the longitudinal joints. Besides this, the heads, in some cases, being dished are additionally strengthened (the extent of the dishing being equal to a radius of the diameter of the boiler shell), and in the case of flue boilers the heads are strengthened by the flues and tee-iron (T) girders on the flat surfaces above the flues connected by rods generally attached to the centre shell plate, but better still when

stayed from head to head connected by turnbuckles, so it will be seen that the weakest points are the longitudinal seams.

In all boiler joints it is important that the rivet holes in the various plates and braces should coincide perfectly with each other, and that they should be completely filled by the rivets. If they do not all correspond accurately they should never be forced to do so by the use of a drift-pin. Rivets should be placed in those rivet holes which agree properly, and the remainder should then be reamed out until the holes coincide.

The strong competition in boiler making, and the ruling low prices at which work is contracted for to be delivered, have caused great carelessness in this industry. In my experience as a boiler inspector, I have found boilers made by parties who stand high in the trade in which the plates were not punched to properly coincide: so little so that they formed eccentric openings to receive the rivets through which a drift-pin had been driven to allow the rivet to enter. The use of the drift-pin is a relic of barbarism; it not only overstrains the boiler plate, but inclines the hole so that the rivet will not be at right angles to the plate. The strength of such a rivet may be *only twenty per cent.* of that of a driven rivet in properly-punched plates. A drift pin should *never* be used. When rivet holes do not coincide they should be reamed until they do, and then rivets of proper size to fill them should be used.

Commercially, boiler-plates cannot be drilled or punched with sufficient accuracy to coincide with one another, therefore, all rivet holes should be punched (say) at least one-sixteenth of an inch less in diameter than the rivet's diameter after being driven, and when all the plates are brought well together in place, by temporary bolts, the holes should be reamed to final rivet diameter by a *fluted* reamer—not the common half-round reamer in general use in a majority of boiler shops. With rivet holes properly spaced, punched or drilled, and reamed as described above, with the outside and inside of the hole slightly countersunk by a *die* —not cut away—the value of the joint may be safely

trusted at sixty-five per cent. of the plate value for single and eighty per cent. for double-riveted joints.

The fact that the greatest strength of any structure is only equal to that of its weakest part is equally true of steam boilers.

I will, therefore, give my practice in securing the strongest practical joint. The proper proportion of the seams of boiler shells to obtain the greatest strength is the aim of all engineers.

In the majority of boilers commercially made the rivet-section area exceeds that of the plate-section area as much as *forty per cent.* This should not be so.

It is well known that the majority of plates used for boilers is weakened by the amount of material punched out, and that that which is left after punching is very much deteriorated, around the holes, if inferior plates are used, by the strain to which the metal has been subjected, on account of the large clearance allowed by a majority of boiler-makers between the diameter of the punch and that of the bolster or die.

The effects of the strains to which plates are subjected, due to the large clearance allowed, as well as from the use of inferior boiler plates, can be seen by examining any ordinary punched plate rivet hole. The material always has a peculiar granular appearance, showing that the fibre has been destroyed; and by taking a piece of boiler plate which has been punched, planing it through the centre of the punched holes and rivets, and then polishing the section and applying dilute muriatic acid, the effect of the punch upon the fibres of the iron can distinctly be seen.

In addition to holding boiler plates together, rivets have another function to perform, which is to keep the joints water- and steam-tight. To do this they must be placed near enough to each other to hold the plates together, thus preventing them from leaking between the rivets. If the rivets do not fill the holes, there is also danger of their leaking around the heads. Now, a badly fitted or crooked rivet, obviously, will not hold the plates together so well as one which is perfectly straight and fits the hole accurately. Anyone who has ever observed two plates which have been

punched rivet size (by this I mean the size the rivet becomes after being driven, the usual practice being to punch the holes one-sixteenth or 0·0625 of an inch greater in diameter than the diameter of the rivet to be used), and placed together, knows it is impossible to make the holes correspond with any degree of accuracy, and that they must either be strained into position by driving a drift-pin into the holes or else reamed out to get the rivet to enter. The manner in which rivets are practically fitted will be seen from the following, *Figs.* 1 and 2, made from pieces of old boilers, planed through the centre line of the rivets.

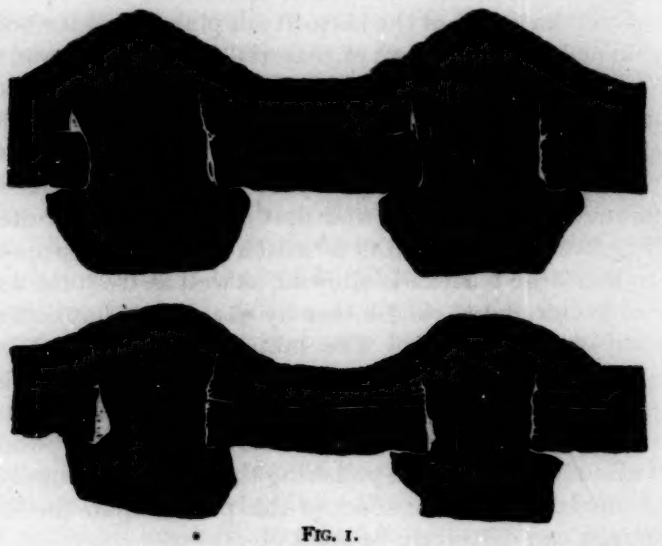

FIG. 1.

From these sections impressions with ordinary printer's ink were transferred to blocks and engraved, therefore, they may be relied upon as entirely accurate. They are by no means examples of the worst specimens, as they were selected at random from old boilers. The engraver has endeavored to reproduce the appearance of the fibre of the iron caused by the punch passing through it. It will be seen that the metal for a space of from one to three-sixteenths of an inch around the hole has been "fatigued," or strained, so that apparently little of its strength is left. Of

course, where iron is subjected to so great a strain, besides injuring its fibre, there is danger of cracking it and thus creating a flaw. It will also be seen that the holes do not correspond with each other, and that, therefore, some of the rivets are crooked and some do not fill the holes.

It will also be seen by *Figs. 1* and *2* that the rivets are fitted so badly into the holes that they would evidently resist very little shearing strain, which must be taken by the adjoining rivets. It is true that when plates are held together there will be considerable friction between them, but this friction may not act simultaneously with the

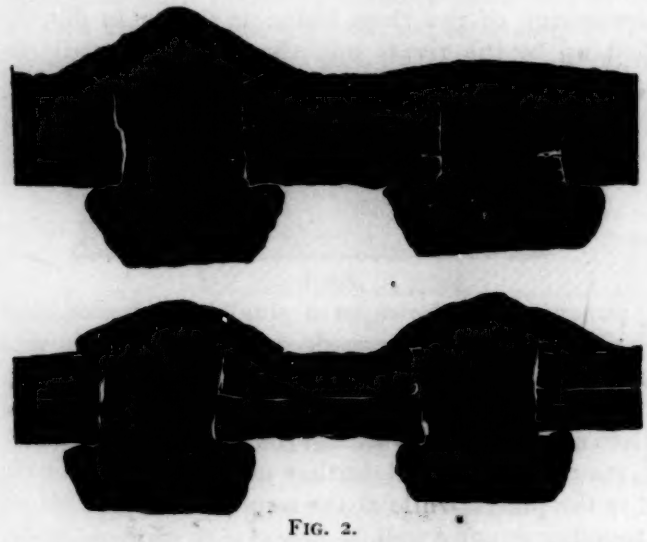

FIG. 2.

resistance to shearing, owing to irregularity of surfaces, so that the force acting upon the boiler plates might be resisted first by the rivets and then by the friction of the plates upon each other. The total strength of the plates is, of course, less, separately, than when taken together. Now, if the holes were fair and the rivets driven into the holes properly, the strain on the seam would be resisted equally by all of the rivets conjointly, instead of by a portion only, and also by the maximum friction of the plates.

To get the best result, I have, in my practice, specified in

boiler contracts that all rivet holes shall be punched the size of the rivet to be used and that when the plates are in their respective positions, held by temporary bolts, the holes shall be reamed one-sixteenth of an inch diameter with a fluted reamer, after which the inside and outside of shell plate shall be countersunk by a steel die (not cut away by a sharp-edged tool).

By the above process the annular ring around the rivet hole, due to detrusion of the punch, is removed, the metal which is left between the holes of the plate is uninjured, and consequently is much stronger than would be the case if the holes had been punched or drilled full rivet size. The countersinking of the rivet holes, as shown in *Fig. 3*, will be filled up by the rivets and thus add materially to the strength of the latter at their weakest point.

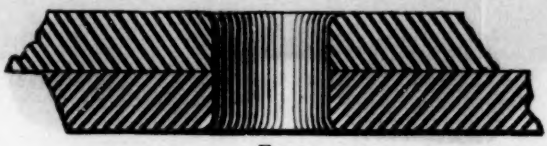

FIG. 3.

By punching the holes of a smaller diameter than the driven rivet size, and afterwards reaming the same in place, the minimum reduction in area of the plate by punching will be attained, and consequently the maximum strength of the complete structure; and as the rivets will be much better fitted, they can be spaced farther apart, thus leaving more metal in the plates, while at the same time the latter will be held together equally well.

With the holes punched small and reamed to full rivet size, they will always conform exactly with each other, even with careless workmanship, so that the danger to which we are always liable with boilers whose rivet holes have been punched or drilled—that of having undue strain fall upon some of the rivets—will be obviated, and at the same time the metal left between the holes will have more strength than when punched or drilled in the usual way; the danger of leakage around the rivets will be obviated to a great extent and, therefore, they can also be spaced farther apart.

By increasing the diameter of the rivets, their transverse sectional area is increased much more rapidly than the longitudinal section of the holes they must fill, so that giving them sufficient strength does not involve taking away an equal sectional area of the plates.

Besides the weakening of the plate by the strain to which it is subjected by careless punching, there is always a degree of uncertainty about the strength of seams if the plates are punched to full rivet size, which must be a fruitful cause of weakness and consequent disaster. Every engineer, boiler-maker or person who has made even the most casual observation in a boiler shop, knows that as the holes are laid out and punched separately to full rivet size, they never *correspond exactly*, however great the care taken in spacing. Frequently they all come obliquely and must either be reamed out for a larger rivet, or else the

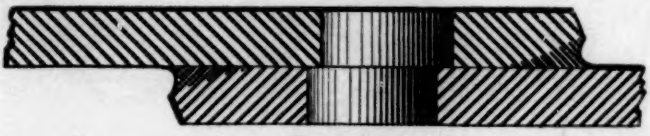

FIG. 4.

plates are strained by driving in a drift-pin to force them in position, in order to bring the holes near enough over each other, to allow the rivet to enter. That they do not fill the holes when the plates are in such a position is distinctly shown if a seam be planed through the centre line of the rivets, as shown in *Figs. 1* and *2*.

Now, suppose that some of the holes in the two plates come in relation to each other, as they often do, in the manner represented in *Fig. 4*. In such an event it will be almost impossible to make the rivet fill the holes unless the latter are reamed out much larger than the size they are punched. If this is done a larger sized rivet must be used, which can be made to fit accurately, but entire dependence must be placed on the faithfulness of the workman employed to do this—a reliance, the uncertainty of which only those who have had experience know, and which, I regret to say, of late years seems to be growing more and more insecure.

Now, the fact that workmen *could* do good work if they would, does not lessen the danger from this source. In making boilers with rivet holes punched to full rivet size there is always a risk of bad workmanship, and, what is worse, it is always out of sight and therefore undiscoverable.

With boiler plates carelessly punched to full rivet size there is also danger that some of the rivets will be subjected to an undue strain. If, for example, we have three rivets, as shown in *Fig. 5*, of which only the centre holes correspond with each other, and consequently are the only ones which are fitted accurately by a rivet, all the others will fill the holes probably somewhat, as in *Figs. 1* and *2*, or as shown in *Fig. 6*. Now, if the middle rivet in *Fig. 5* should fit accurately, while

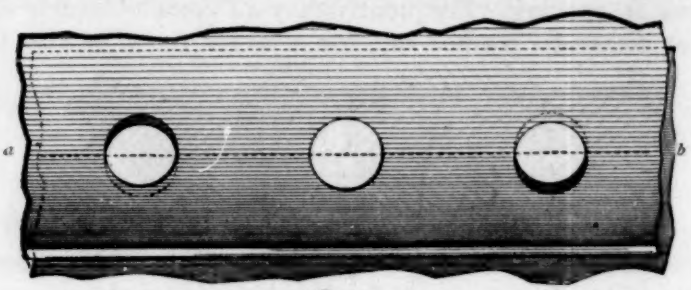

FIG. 5.

all the others should fit, as shown in *Fig. 6* (which probably frequently does occur), then the centre rivet must bear all the strain until the others are drawn up to a bearing. There would, of course, under such circumstances, be very great danger of shearing off the rivet or, of starting a fracture in the sheet, as indicated by the dotted line *a b*, in *Fig. 5*. When once started the constant expansion and contraction from the strain in the boiler would, of course, have a tendency to increase it, until there would not be sufficient strength left to resist the steam pressure, and a "mysterious" (?) explosion would result.

Therefore, I claim that just so long as boiler plates are punched or drilled full rivet size there will be difficulty in getting the holes to correspond exactly with each other, and danger from bad workmanship will exist.

This difficulty and danger can be entirely overcome by using the best material and punching the rivet holes one-sixteenth of an inch less than full rivet size, with an excess of plate area over that of the rivet, and, when the parts are in place, reaming the rivet holes to full rivet size; those portions of the plate left between the holes will then be from *ten* to *twenty per cent.* stronger than the commercially made boilers at the present time.

The additional cost of making boiler joints in the manner above described, is insignificant as compared with the practice in vogue, and in view of the great advantages gained. To illustrate, the additional cost of an eighty horse-power boiler constructed by one of our leading boiler-makers, in the manner contended for, was only fifteen (15) dollars.

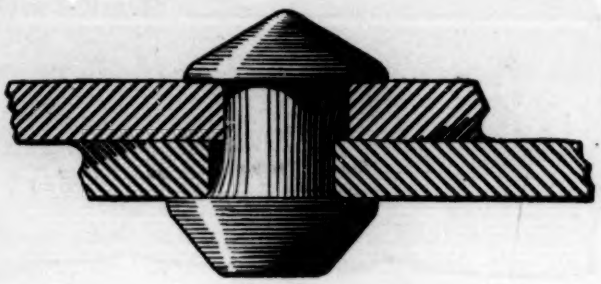

FIG. 6.

When we take into consideration the advantages gained by the buyer in having a better-made structure, which will be capable of doing more work by the increased steam pressure, at which it will be practicable to run it, the increase in cost will be a good investment.

From the foregoing, it will be seen that a single riveted joint, made as described, will be twenty per cent. stronger than the ordinary riveted plates having the rivet holes punched or drilled full rivet size. As has been shown, this difference in strength is attributable partly to the injury done to the plates by punching and to the presence of the injured parts. When the holes are punched of a smaller diameter than full rivet size, and afterward reamed to the latter dimensions, the reaming removes all the portions

injured by the punch. Furthermore, all the rivet holes coincide exactly with each other, and, therefore, the rivets will fit perfectly and will also be less likely to leak. For this reason the rivets can be spaced farther apart, and, by increasing their diameter slightly, their sectional area will be sufficiently great to equal that of the plate metal left between the holes, as shown in *Fig. 7*, composed of three-eighths-inch plates.

The objection raised, as before stated, to spacing the rivets for three-eighths-inch plates, as shown in *Fig. 7*, is that the seams are liable to leak between the rivet holes. In other words, the plates will not be held together tight enough to prevent the forcing of steam and water between them. The greatest distance that rivets may be spaced

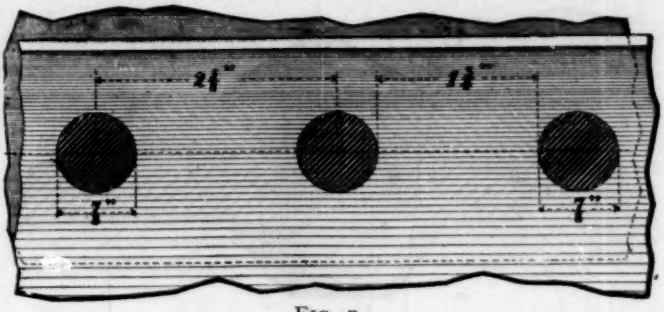

FIG. 7.

apart without incurring danger of leakage between the plates must, however, be determined more by practical than theoretical considerations. What I wish to make clear is, that a boiler joint will be stronger if the diameter and distance apart of the rivets are increased in the proper proportion to each other.

There is, however, a limit beyond which the diameter of rivets cannot be increased with advantage, because if we increase their diameters, their sectional area to resist shearing is increased in proportion *to the square of the diameter*, whereas the section of metal in the plate to resist crushing is increased only *as the distance between the holes.*

A given increase to the diameter of the rivets will therefore increase their sectional area much more than the

material of the plates will be cut away by enlarging the rivet holes.

This will be apparent if we compare a rivet one-half inch in diameter with one one inch in diameter. The first has a sectional area of 0·1963 square inch, the other 0·7854 square inch, or *four times* that of the first one. Now the area which resists the crushing strain of the rivets is increased only in proportion to their diameters, or *twice* as much for the one as for the other. If, therefore, we increase the diameters of the rivets, we very soon reach a point at which the plate has less strength to resist crushing than the rivet has to resist shearing. The diameter of a rivet which will give just the same resistance to both strains varies with the thickness of the plates. With three-eighths-inch thick plates a seven-eighths-inch diameter rivet will have a resistance to shearing of about 30,000 pounds, and the three-eighths-inch plates in front of it, a resistance to crushing also of about 30,000 pounds. A seven-eighths-inch diameter rivet is, therefore, the largest sized iron rivet which can be used to advantage in three-eighths-inch plates.

If now we were to space such rivets so far apart that the metal left between the rivet holes would equal the area of the rivet in cross section, we would have a strength just equal to that of the rivet; we would have the strongest possible seam or joint that can be made with a single row of rivets.

In plates three-eighths-inch thick, and seven-eighths-inch diameter, rivets of the above value being employed, the distance between the margins of the rivet holes would be one and three-fourths inches, or two and five-eighths inches from centre to centre of rivet holes. (See *Fig.* 7.)

I have been explicit on this point, because the distance of the rivets apart has an important bearing where double-riveted joints are used. The advantages of double-riveted joints over single-riveted joints are that the strain is distributed among a greater number of rivets, and therefore the rivets may be smaller, and consequently less material cut out of the cross section of the plates for the rivet holes.

All engineers are aware of the great advantage gained

in double-riveted joints by placing the rivet holes in two rows alternately to each other, or in spacing them "zigzag" or "staggered," as shown in *Fig. 8*, which represents three-eighths-inch thick plates, which is the thickness in most general use for steam-boiler purposes.

It has been found in practice that for the greatest strength, the distance K between the centre of rivet holes c e, *Fig. 8*, or in the direction of the joint, should be double the distance $\frac{1}{2}K$ between the centre lines of rivet holes of the two rows, and the rivets will then form a right angle or 90° with one another. The distance between the rivet holes in the direction of a c, or the joint, can be made fifty

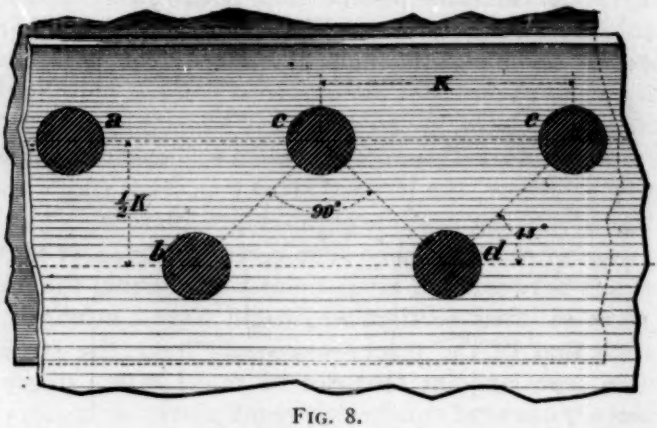

FIG. 8.

per cent. greater than between rivet holes in single-riveted joints. The diagonal distance between centres of rivet holes should be made equal to the distance in the direction of the joint in single riveting.

In regard to spacing rivet holes zigzag, as shown in *Fig. 9*, it will be seen that the rivets in the respective rows are much nearer together than those in the same row, but the sectional area of plate in the line a b c d and e is over two square inches, whereas, in the line f g it is only one and nine-tenths square inches. Therefore, although the rivets on the zigzag line are nearer together than those in the same row, yet the line a b c d and e presents more material to resist rupture. The strongest joint of this kind

will be one in which the material between the rivet holes—say *b d*—in the same line, is equal to two of the spaces *a b* and *b c*, between the rivets in adjoining lines. Or, to express it algebraically, and supposing x to represent the distance between the circumference of the rivet holes *b* and *d* and *m*, and *n* equal to that between *a* and *b*, and *b* and *c*, then we ought to have:

$$x = m\,n$$

In order to make the strongest double-riveted joint of this kind, we must, therefore, first determine upon the greatest distance which is admissible between the rivet holes in order to keep the boiler joint steam- and water-tight. Assuming this distance to be one and three-eighths

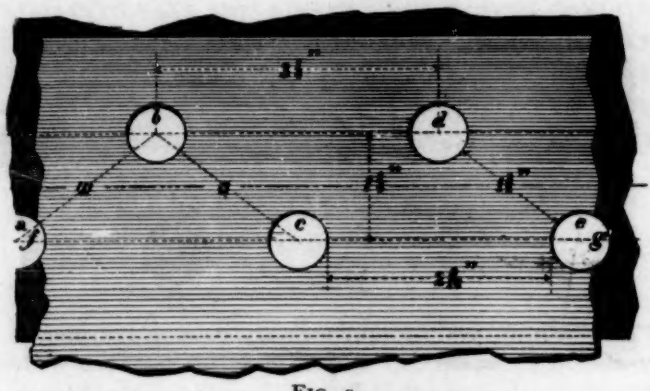

FIG. 9.

inches, as shown in *Fig. 9*, then the spaces between the rivet holes on the *same line* should be two and three-quarters inches. If the plates are punched less than full rivet size, and when in place reamed to full rivet diameter, the sectional area of the rivets should be equal to that of the plates between the rivet holes. (This is assuming that the tensile strength of plates and rivets are in value equal to each other, or nearly so.) As there are two rivets to resist the strain due to the space between the adjoining rivets, as *b d*, for example, therefore, the sectional area of each rivet should be just half that of the plate section between the rivet holes *b d*. With a three-eighths-inch thick plate, this latter area would be one and three-hundredths square inches,

the half of which would correspond very nearly with the area of a thirteen-sixteenths-inch rivet.

Having the distance between the rivet holes on the same line and the diameter of the rivets, it is, of course, easy to establish their pitch, which in this case, would be three and nine-sixteenths inches from centre to centre as represented in *Fig. 10.*

It only now remains to establish the position of the rivet hole *c* in the adjoining line. This can easily be done by taking a radius, *a e* or *b d* equal to one-half the distance between the centres of the rivet holes *a* and *b*, and from them describing two arcs *p e* and *p d*. If, now, the rivet hole *c* be located so as to be tangent to these two arcs, then the

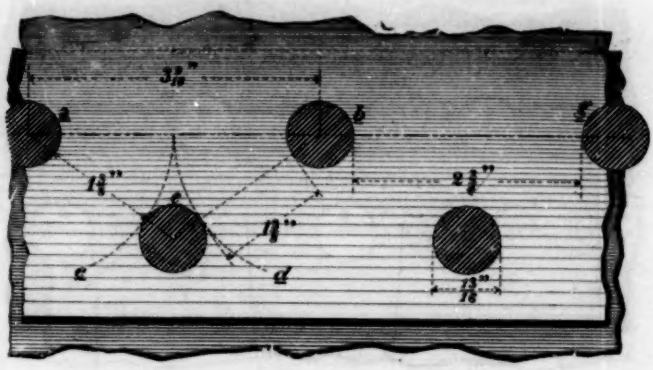

FIG. 10.

spaces between the rivet holes *a c* and *c b*, will be just equal to that between *a* and *b*. The plates will, therefore, have the same strength in the line *a c b* as in *a b*. The centre of the rivet hole *c* can be located, perhaps, more easily, by taking half the pitch *a b*, plus half the diameter of the rivet hole as a radius, and describing from *a* and *b* two arcs intersecting at *c*, which will be the centre of the rivet hole.

In the ordinary course of boiler-making almost every error of workmanship tends to weaken the plates and give an excess of rivet area, while, in addition, the corrosion, which to a greater or less extent takes place during the life of a boiler, also weakens the plate while scarcely affecting the rivets.

It is for these reasons that I consider that a riveted boiler joint should be laid out so as to have an excess of plate section over that of the rivet.

It is impracticable to proportion the riveted joint so perfectly that the shearing strength of the rivet will be equal to the tearing strength of the plate, for the actual strength of the plates varies more than does the proportion of dimensions of the joint.

[*To be continued.*]

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ANALYTICAL DISCUSSION OF THE TIDAL VOLUME

ADMITTED INTO BAYS AND RIVERS UNDER GIVEN CONDITIONS OF WIDTH, DEPTH AND AREA OF BASIN—RELATION BETWEEN WIDTH AND DEPTH IN THE CROSS SECTIONS OF TIDAL STREAMS WHOSE BEDS ARE YIELDING TO THE ACTION OF CURRENTS—VARIATION OF CROSS-SECTIONAL AREA IN SUCH STREAMS.

By L. d'Auria.

If a perfect freedom of flow existed in a tidal river of uniform width and uniform depth, the range of tide in such river would also be uniform, and the profile of the tidal wave could be identified with the curve of sines.

Imagine any portion of this ideal tidal river, from its head down to any given cross section, and let us find the analytical expression of the tidal volume which would flow through such cross section, from slack water to slack water.

In the first place we will observe that slack water in any given cross section of the tidal river under consideration would occur when either the crest or the trough of the tidal wave occupies a position midway between the given cross section and the head of the river. When the crest is in such position, we would have what is generally called *high-water slack*, and when it is the trough, we would have the *low-water slack*, through the cross section.

These two phases evidently do not correspond with high water and low water, but occur some time afterward; and it can be seen that the delay between high water and high water slack, as well as the delay between low water and low-water slack would increase with the distance of the cross section from the river head. Theoretically, in a cross section, of which the distance from the head of the river is equal to one-half the length of the wave, or equal to the distance from crest to trough, the time interval between high water and high-water slack, or low water and low-water slack, would be equal to one-half the duration of the rise or fall of tide, or

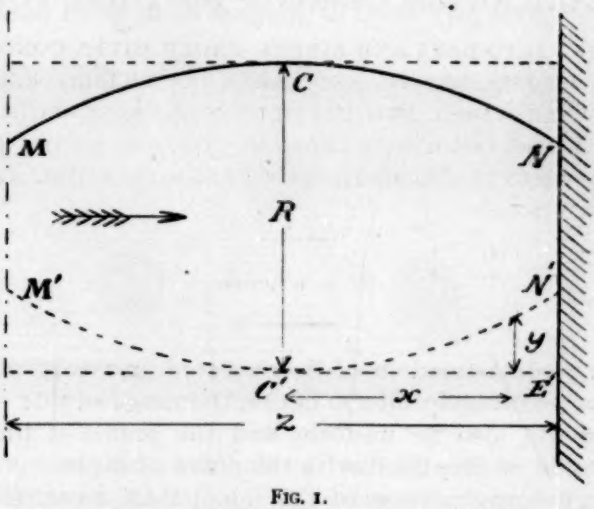

FIG. 1.

a little over three hours. Through such cross section the flood current would continue to flow for all this time after high water, and the ebb current would continue to flow for the same length of time after low water. At any other cross section, either above or below the one already considered, the time interval in question would be shorter. For instance, in a cross section of which the distance from the river head would equal the length of the wave, slack water would occur simultaneously with high and low water, and the same coincidence evidently occurs in the cross section which is right at the head of the river. All these deduc-

tions, though theoretical, represent facts generally observed in tidal rivers in a greater or less degree.

Let MCN and $M'C'N'$ (*Fig. 1*), represent the profiles of the tidal wave in the two positions which correspond to high-water slack and low-water slack, respectively, through the cross section MM', whose distance from the head of the river NN' is z; and let λ denote the length of the wave; R the height from crest to trough, and b the width of the river. It is obvious that the tidal volume passing through the cross section MM' from one slack water to the other will be

$$q = b\,(R\,z - 4 \times \text{area } C'E'N')$$

and the problem is reduced to find the area $C'E'N'$.

Let x and y represent the coördinates of any point of the curve $C'N'$ referred to C' as origin. Then will be found

$$y = R \sin^2\left(\frac{\pi x}{\lambda}\right) \tag{1}$$

and

$$\text{area } C'E'N' = R \int_0^{\frac{z}{2}} \sin^2\left(\frac{\pi x}{\lambda}\right) dx$$

Hence,

$$q = b\,R\left(z - 4\int_0^{\frac{z}{2}} \sin^2\left(\frac{\pi x}{\lambda}\right) dx\right)$$

Integrating between the assigned limits we find, after reduction,

$$q = \frac{\lambda\, b\, R}{\pi} \sin\left(\frac{\pi z}{\lambda}\right) \tag{2}$$

Let T represent the mean tidal interval in seconds or $T = 44{,}600$ seconds, approximately. Then if v denotes the mean velocity of wave propagation in the distance z we can put $\lambda = Tv$, and write.

$$q = \frac{Tv\, b\, R}{\pi} \sin\left(\frac{\pi z}{Tv}\right) \tag{3}$$

This expression becomes a maximum when $z = \frac{1}{2}\, v\, T$, which would show that the greatest tidal volume which can

be admitted into a tidal basin whose width is b and whose length exceeds the quantity $\frac{1}{2} v T$ is

$$Q = \frac{T v b R}{\pi} \tag{4}$$

since it would be impossible from a physical point of view to admit that by increasing the length of the tidal basin beyond the limit $\frac{1}{2} v T$, a diminution of tidal volume would ensue. Hence, from such standpoint equation (4) ought to hold good for all cases where $z = > \frac{1}{2} v T$, while equation (3) ought to be applied to cases where $z < \frac{1}{2} v T$.

Now it may be reasonably assumed that two tidal basins of equal area but different shapes, having the same width of entrance, the same average depth and the same range of tide at the entrance would admit an equal tidal volume during each tide. With this assumption, if we denote the area of the basin by A, we can put $z = A \div b$ in equation (3), which then becomes

$$q = \frac{T v b R}{\pi} \sin\left(\frac{\pi A}{T v b}\right) \tag{5}$$

with the limitation of $A < \frac{1}{2} v T b$; and when $A = > \frac{1}{2} v T b$, we have to apply equation (4).

In cases where the widths of the cross sections change from low to high water, the area A of the tidal basin will have to be computed at low water, and the width b at mid-tide. In order to appreciate the correctness of this remark, imagine first a tidal basin of uniform width from low to high water. Now, if without altering the cross section of the entrance to such basin we were to increase gradually the widths of all the other cross sections from low to high water without touching the low-water widths, it can hardly be expected that by such operation the tidal volume would be increased, although a considerable increase in the mean area of the basin has taken place. Such increase would evidently cause a reduction of tidal range in the upper cross sections, and this would compensate for the increased mean area of the basin, as far as the tidal volume is concerned.

If we denote by β the mean low water width of the tidal basin, by l its length, and by θ the average of the time

intervals of high water and low water in the distance l, we can put equation (5) under the form

$$q = \frac{T l b R}{\pi \theta} \sin\left(\frac{\theta \beta \pi}{T b}\right) \qquad (6)$$

In order to test this formula, let us apply it to a well-known case where the necessary data have been well determined by observations. This case we find in the portion of the Delaware River, between Bridesburg (above Philadelphia) and Trenton, for which we have $l = 144{,}000$ feet; $b = 3{,}860$ feet; $\beta = 1{,}764$ feet; $R = 5{\cdot}98$, and $\theta = 162$ minutes. Putting $T = 762$ minutes, we obtain by substituting in formula (6)

$$q = 1{,}496{,}000{,}000 \text{ cubic feet.}$$

Now, according to an extremely accurate measurement of the tidal volume in question deduced from simultaneous observations, made by the United States Engineers, it has been found 1,452,000,000 cubic feet, which is only about three per cent. less than the theoretical.

It may be that taking into account the volume of fresh water discharged by the river, this difference would be somewhat larger, but we only claim to give the theoretical, not the actual tidal volume.

When the basin is shaped like a bay, with narrow entrance, it is rather difficult to obtain the proper values of θ and l, which are required by formula (6), to compute the tidal volume. However, if we knew the value of v in equation (5), we could use this equation instead of the (6) in computing the tidal volume, independently of θ and l. Now, if we denote by (H_0) the average depth of the basin at mid-tide, we can put approximately

$$v = 4{\cdot}81 \sqrt{(H_0)}$$

[See "A new theory of the propagation of waves in liquids. By the author. *Journal Franklin Institute*, December, 1890.]

Then we can write:

$$q = \frac{4{\cdot}81\, b\, T R \sqrt{(H_0)}}{\pi} \sin\left(\frac{\pi A}{4{\cdot}81\, b\, T \sqrt{(H_0)}}\right) \qquad (7)$$

When

$$A = > \tfrac{1}{2}\, 4{\cdot}81\, b\, T \sqrt{(H_0)}$$

which is the case of very large bays with narrow entrances, then, instead of equation (4), we can use

$$Q = \frac{4{\cdot}81\, b\, T\, R \sqrt{(H_0)}}{\pi} \qquad (8)$$

In order to test formula (7) we have taken the case of Absecom and Brigantine Bay, Atlantic City, N. J., for which we have the following data: $A = 362{,}400{,}000$ sq. ft.; $b = 1760$ ft.; $(H_0) = 9{\cdot}5$ ft.; $R = 4$ ft. Substituting these data in equation (7) we find

$$q = 1{,}482{,}000{,}000 \times \sin 56^\circ$$

or

$$q = 1{,}228{,}000{,}000 \text{ cubic feet.}$$

By gauging, and with the same range of tide of four feet, Mr. George Daubeney, Assistant Engineer, found in 1880 a tidal volume equal to 1,174,612,000 cubic feet [see *Report of Chief Engineers U. S. Army*, 1881]; and in 1886, by simultaneous tidal observations with the same range of tide was found again, by the writer a tidal volume equal to 1,170,614,000 cubic feet. [See *Report of Chief Engineers U. S. Army*, 1887.]

Both these measurements, surprisingly near to each other, are in the mean only four per cent. less than the theoretical tidal volume computed by formula (7). Such close agreement, we believe, entitle us to view our formulæ from a physical point and trust the conclusions which may thus be reached. Now we may readily see from formula (8) that in very large tidal basins with narrow entrances the tidal volume is directly proportional to the width of entrance, showing that in large tidal bays it would be useless to change the width of entrance with a view to increase the velocity of the currents through such entrance; and as far as the upper navigation of such bays is concerned, it teaches that by narrowing the width of entrance we cut away from the various channels an amount of flow proportionately to the reduction of width of entrance effected.

In tidal rivers, however, where formula (7) is applicable, this formula would show that the tidal volume varies with the quantity

$$\psi = b \sin\left(\frac{\pi A}{4{\cdot}81\ b\ T\sqrt{(H_0)}}\right) \qquad (9)$$

other things remaining the same. Now, it can be seen that this quantity is only feebly affected by b when the area of the basin A is small compared with

$$2{\cdot}4\ b\ T\sqrt{(H_0)}$$

In fact, suppose for instance that we should find

$$\frac{A}{2{\cdot}4\ b\ T\sqrt{(H_0)}} = \tfrac{1}{3}$$

then the tidal volume would be proportional to $\frac{1}{2}\ b$. If instead of b we substitute, say $\frac{2}{3}\ b$ in (9), then the tidal volume would become proportional to $0{\cdot}47\ b$, showing, therefore, a loss of only six per cent. of tidal volume against a reduction of thirty-three per cent. of the width of entrance.

Let ψ_1 represent the value of ψ corresponding to the width $b_1 < b$; then the loss of tidal volume incurred would be expressed by

$$j = \left(1 - \frac{\psi_1}{\psi}\right) q$$

In the above argument we have tacitly supposed that the reduction of width amounts to a contraction of the river at a particular point, forming a kind of bay above it; but where only a rectification of the banks of the river is contemplated no loss of tidal volume can possibly follow.

In projecting improvements of tidal rivers it has always been a perplexing question how far the tidal volume might be affected. We can answer this question now, fully, by consulting our formulæ (9) and (10), when the necessary data are given; and in a general way we have learned that where the ratio

$$A \div 2{\cdot}4\ b\ T\sqrt{(H_0)}$$

is considerably smaller than *one*, say $\frac{1}{4}$ or $\frac{1}{3}$, which would be the case of the upper sections of tidal rivers, then a contraction of width may be effected, practically without loss of

tidal volume, a conclusion which may save a great amount of discussion relating to the improvement of such sections of tidal rivers.

In tidal rivers whose beds are yielding to the action of currents the depth of the cross sections must bear some relation to the mean velocity of the stream. Let u represent this velocity through a cross section Ω whose depth is H, and put $H = cu^n$, in which n is to be determined, and c is a constant. Now, from formula (7), we can compute

$$u = \frac{4{\cdot}81\ R\sqrt{(H_0)}}{\pi H} \sin\left(\frac{\pi A}{4{\cdot}81\ b\ T\sqrt{(H_0)}}\right) \tag{10}$$

and consequently we find

$$H = c_1 R^{\frac{n}{n+1}} (H_0)^{\frac{n}{2(n+1)}} \sin^{\frac{n}{n+1}}\left(\frac{\pi A}{4{\cdot}81\ b\ T\sqrt{(H_0)}}\right) \tag{11}$$

This formula when applied to the Delaware River led the writer to the conclusion $n = 2$, hence

$$H = c_1 R^{\frac{2}{3}} (H_0)^{\frac{1}{3}} \sin^{\frac{2}{3}}\left(\frac{\pi A}{4{\cdot}81\ b\ T\sqrt{(H_0)}}\right) \tag{12}$$

Changing the width b of the cross section Ω, this formula offers the corresponding mean depth when other things are given. Let, for instance, b_1 and H_1 represent the new width and the new depth, we would have

$$\frac{H_1}{H} = \left(\frac{R_1}{R}\right)^{\frac{2}{3}} \cdot \frac{\sin^{\frac{2}{3}}\left(\frac{\pi A}{4{\cdot}81\ b_1\ T\sqrt{(H_0)}}\right)}{\sin^{\frac{2}{3}}\left(\frac{\pi A}{4{\cdot}81\ b\ T\sqrt{(H_0)}}\right)} \tag{13}$$

When A is small compared with

$$2{\cdot}4\ bT\sqrt{(H_0)}$$

and the ratio does not exceed, say $\frac{1}{2}$, we can write simply

$$\frac{H_1}{H} = \left(\frac{R_1}{R}\right)^{\frac{2}{3}} \left(\frac{b}{b_1}\right)^{\frac{2}{3}} \tag{14}$$

If we multiply equation (12) by b we have

$$\Omega = c_1\, b\, R^{\frac{2}{3}} (H_0)^{\frac{1}{3}} \sin^{\frac{2}{3}}\left(\frac{\pi A}{4{\cdot}81\ b\ T\sqrt{(H_0)}}\right) \tag{15}$$

or, remembering that we have expressed the velocity of propagation by

$$v = 4{\cdot}81 \sqrt{(H_0)}$$

and that $A = z\beta$ in which z is the length and β the average width of the basin, we can readily find

$$\Omega = c_2\, b\, R^{\frac{2}{3}} \left(\frac{z}{\theta}\right)^{\frac{2}{3}} sin^{\frac{2}{3}} \left(\frac{\beta\,\theta\,\pi}{b\,T}\right) \tag{16}$$

For the upper portion of a tidal river we find θ considerably smaller than T, and therefore, for such case, we can assume the sine proportional to

$$\left(\frac{\beta\,\theta}{b}\right)$$

and write

$$\Omega = c_3\, R^{\frac{2}{3}}\, z^{\frac{2}{3}}\, \beta^{\frac{2}{3}}\, b^{\frac{1}{3}} \tag{17}$$

and

$$\frac{\Omega_1}{\Omega} = \left(\frac{R_1\,\beta_1}{R\,\beta}\right)^{\frac{2}{3}} \left(\frac{z_1}{z}\right)^{\frac{2}{3}} \left(\frac{b_1}{b_1}\right)^{\frac{1}{3}} \tag{18}$$

When the cross sections are not too far apart we can consider the mean width of the basin and the range of tide to remain constant, and then we have

$$\frac{\Omega_1}{\Omega} = \left(\frac{z_1}{z}\right)^{\frac{2}{3}} \left(\frac{b_1}{b}\right)^{\frac{1}{3}} \tag{19}$$

In order to test this relation we have selected three pairs of cross sections from the Delaware River, each pair exhibiting a great variation of width in a short distance, and the results are as follows:

Fisher's Point:

$\Omega = 52{,}927$ sq. ft.; $b = 2{,}610$ ft.; $z = 144{,}000$ ft.

Elevator Wharf:

$\Omega_1 = 67{,}380$ sq. ft.; $b_1 = 4{,}240$ ft.; $z_1 = 154{,}000$ ft.

$$\frac{\Omega_1}{\Omega} = 1{\cdot}27\,; \left(\frac{z_1}{z}\right)^{\frac{2}{3}} \left(\frac{b_1}{b}\right)^{\frac{1}{3}} = 1{\cdot}23.$$

Gloucester:

$\Omega = 53{,}625$ sq. ft.; $b = 1{,}800$ ft.; $z = 186{,}000$ ft.

Horse Shoe:

$$\Omega_1 = 91{,}380 \text{ sq. ft.}; \; b_1 = 5{,}850 \text{ ft.}; \; z_1 = 196{,}000 \text{ ft.}$$

$$\frac{\Omega_1}{\Omega} = 1{\cdot}70; \left(\frac{z_1}{z}\right)^{\frac{2}{3}} \left(\frac{b_1}{b}\right)^{\frac{1}{3}} = 1{\cdot}53.$$

Deep Water Point:

$$\Omega = 140{,}500 \text{ sq. ft.}; \; b = 4{,}860 \text{ ft.}; \; z = 326{,}000 \text{ ft.}$$

New Castle:

$$\Omega_1 = 182{,}200 \text{ sq. ft.}; \; b_1 = 8{,}400 \text{ ft.}; \; z_1 = 344{,}000 \text{ ft.}$$

$$\frac{\Omega_1}{\Omega} = 1{\cdot}30; \left(\frac{z_1}{z}\right)^{\frac{2}{3}} \left(\frac{b_1}{b}\right)^{\frac{1}{3}} = 1{\cdot}24.$$

On examination it will be found that at Gloucester the bottom is rather hard compared with the other cross section, a circumstance which seems to account for the difference of ten per cent. found in the second case between the actual and the theoretical result. The small difference found in the other two cases, though utterly insignificant in computations of this kind, still is in a direction which is in accordance with the factor neglected in formula (18). Had this factor been computed a still closer agreement would have been found between the results of observation and those obtained by theory.

On a Maximum Steam-Jacket Efficiency.

By Robert H. Thurston.

(1) Ideal Efficiency.—The fact is sufficiently well known that the steam jacket, as employed on the steam engine, of whatever form and arrangement, is intrinsically a wasteful element, and that its use only gives, in certain cases, an economical advantage by its repression of wastes of larger magnitude. It checks a serious and unavoidable waste, more or less completely, by a process which as inevitably involves a waste which is commonly, but, perhaps, not invariably, a lesser one. The ideal steam engine, such as is treated of in the purely thermodynamic study of the steam engine, has a lower efficiency with, than it has with-

out, a jacket. This is readily seen from the following illustrations computed and checked by Messrs. Hitchcock and Mount, at the suggestion of the writer. Nine cases are examined, in which the same initial pressure is taken and the efficiencies are computed both with and without jacket for various ratios of expansion. The results, as given in the following tables and as illustrated in the curves plotted from them, show that the jacketed engine is always more wasteful than the ideal, unjacketed engine. This is sufficiently evident, *à priori*, from the consideration that the latter receives all its steam at a maximum temperature, expands it adiabatically to a certain terminal temperature, and then exhausts it; while the former receives a part of its heat at intermediate temperatures, expands the fluid non-adiabatically, and finally rejects it at the terminal temperature, with a lower mean range of expansion. In other words, the jacketed engine departs furthest from the principles of economical operation first enunciated by Carnot:* All heat should be received at maximum temperature; expansion should be perfectly adiabatic, and should continue to the minimum temperature and pressure, and all should be rejected as nearly as possible at that minimum. Thus, "theoretically," if the use of that much-abused term may be permitted in this sense, the unjacketed engine is more efficient than the jacketed engine.

"Practically," however, the reverse is usually, though probably not always, the case, and the use of the jacket is often found to be productive of a real, and sometimes of large, economy. It is thus obvious that the advantages of the employment of the jacket come of those conditions which distinguish so markedly the real from the ideal case in steam-engine economy; those which make the "Theory of the Real Engine," as the writer has called it, essentially different, in important respects, from the "Theory of the Ideal Engine."†

* *Reflections on the Motive-Power of Heat* By Sadi Carnot. Edited by R. H. Thurston. New York: J. Wiley & Sons. 1890.

† *Development of the Philosophy of the Steam Engine.* Thurston, N. Y.: J. Wiley & Sons. 1889.

The following illustrates this comparison:

EFFICIENCY OF STEAM—IDEAL CASES. RANKINE.

Non-conducting cylinder.

p_1 = absolute pressure of admission;
p_2 = absolute pressure at end of expansion;
p_3 = mean back absolute pressure;
τ_1 = absolute temperature of p_1;
τ_2 = absolute temperature of p_2;
τ_4 = absolute temperature of feed water.

Then the energy per cubic foot of steam admitted:

$$U D_1 = J D_1 \left\{ \tau_1 - \tau_2 \left(1 + hyp\ log \frac{\tau_1}{\tau_2} \right) \right\} + \frac{\tau_1 - \tau_2}{\tau_1} L_1 + r (p_2 - p_3)$$

r = ratio of expansion;
D_1 = density of steam at p_1;
L_1 = latent heat of steam at p_1.

Heat expended per cubic foot of steam admitted:

$H_1 D_1 = J D_1 (\tau_1 - \tau_4) + L_1$;
H_1 = latent heat of evaporation;

$$\text{Eff. of steam} = \frac{U}{H_1}$$

Cylinder Jacketed.

p_1 = absolute pressure of admission;
p_2 = absolute pressure at end of expansion;
p_3 = mean absolute back pressure;
τ_1 = absolute temperature of p_1;
τ_2 = absolute temperature of p_2;
V_1 = volume of 1 lb. steam at p_1;
V_2 = volume of 1 lb. steam at p_2;
a = 1109550 ft. lbs.;
b = 540·4 ft. lbs. per degree F.

Then energy exerted:

$$U^1 = a\ hyp\ log \frac{\tau_1}{\tau_2} - b (\tau_1 - \tau_2) + V_2 (p_2 - p_3)$$

Whole expenditure of heat per lb. steam:

$$h = J(\tau_2 - \tau_4) + a\left(1 + hyp\ log\ \frac{\tau_1}{\tau_2}\right) - b\,\tau_1$$

Eff. of steam:

$$Eff. = \frac{U^1}{h}$$

COMPUTATIONS—IDEAL CASE.

Without Jacket.

Case 1.

$p_1 = 115$ lb. at. $\tau_1 = 799$ $r = 20$
$p_2 = 5{\cdot}32$ $\tau_2 = 626{\cdot}4$
$p_3 = 4$

$$U_1 D_1 = 772 \times {\cdot}2627 \left\{ 799 - 626{\cdot}4 \left(1 + hyp\ log\ \frac{799}{6264}\right)\right\}$$

$$+ \frac{799 - 626{\cdot}4}{799} {\cdot}177760 + 20\,(5{\cdot}32 - 4)\,144 = 46478$$

$$H_1 D_1 = 772 \times {\cdot}2627\,(799 - 561{\cdot}2) + 177760 = 225986$$

$$Eff. = \frac{U_1 D_1}{H_1 D_1} = \frac{46478}{225986} = {\cdot}2073$$

Case 2.

$p_1 = 115$ $\tau_1 = 799$ $r = 10$
$p_2 = 8{\cdot}907$ $\tau_2 = 648{\cdot}5$
$p_3 = 4$

$$U_1 D_1 = 722 \times {\cdot}2627 \left[799 - 648{\cdot}5 \left(1 + hyp\ log\ \frac{799}{648{\cdot}5}\right)\right]$$

$$+ \frac{799 - 648{\cdot}5}{799} {\cdot}177760 + 10\,(8{\cdot}907 - 4)\,144 = 43715$$

$$Eff. = \frac{43715}{225986} = {\cdot}1934$$

Case 3.

$p_1 = 115$ $\tau_1 = 799$ $r = 6{\cdot}66$
$p_2 = 13{\cdot}98$ $\tau_2 = 670{\cdot}7$
$p_3 = 4$

$$U_1 D_1 = 772 \times {\cdot}2627 \left[799 - 670{\cdot}7 \left(1 + hyp\ log\ \frac{799}{626{\cdot}4}\right)\right]$$

$$+ \frac{799 - 670{\cdot}7}{799} {\cdot}177760 + 6{\cdot}66\,(13{\cdot}98 - 4)\,144 = 40481$$

$$Eff. = \frac{40481}{225{\cdot}986} = {\cdot}1795$$

Case 4.

$p_1 = 115$ $\tau_1 = 799$ $r = 4$
$p_2 = 24{\cdot}64$ $\tau_2 = 700{\cdot}2$
$p_3 = 4$

$$U_1 D_1 = 772 \times {\cdot}2627 \left[799 - 700{\cdot}2 \left(1 + hyp\ log \frac{799}{700{\cdot}2} \right) \right.$$

$$\left. + \frac{799 - 700{\cdot}2}{799} {\cdot}177760 + 4\,(24{\cdot}64 - 4)\,144 = 35305 \right.$$

$$Eff. = \frac{35305}{225986} = {\cdot}1566$$

Case 5.

$p_1 = 115$ $\tau_1 = 799$ $r = 2{\cdot}857$
$p_2 = 35{\cdot}85$ $\tau_2 = 721{\cdot}7$
$p_3 = 4$

$$U_1 D_1 = 772 \times {\cdot}2627 \left\{ 799 - 721{\cdot}7 \left(1 + hyp\ log \frac{799}{721{\cdot}7} \right) \right\}$$

$$+ \frac{799 - 721{\cdot}7}{799} {\cdot}177660 + 2{\cdot}857\,(35{\cdot}82 - 4)\,144 = 30700$$

$$Eff. = \frac{30700}{225986} = {\cdot}1358$$

Case 6.

$p_1 = 115$ $\tau_1 = 799$ $r = 2{\cdot}22$
$p_2 = 47{\cdot}41$ $\tau_2 = 738{\cdot}8$
$p_3 = 4$

$$U_1 D_1 = 772 \times {\cdot}2627 \left\{ 799 - 738{\cdot}8 \left(1 + hyp\ log \frac{799}{738{\cdot}8} \right) \right.$$

$$\left. + \frac{799 - 738{\cdot}8}{799} {\cdot}177760 + 2{\cdot}22\,(47{\cdot}41 - 4)\,144 = 27959 \right.$$

$$Eff. = \frac{27959}{225986}$$

Case 7.

$p_1 = 115$ $\tau_1 = 799$ $r = 1{\cdot}818$
$p_2 = 39{\cdot}19$ $\tau_2 = 752{\cdot}6$
$p_3 = 4$

$$U_1 D_1 = 772 \times \cdot 2627 \left[799 - 752{\cdot}6 \left(1 + hyp\ log \frac{799}{752{\cdot}6} \right) \right]$$

$$+ \frac{799 - 752{\cdot}6}{799} \times 177760 + 1{\cdot}818\ (59{\cdot}19 - 4)\ 144 = 25298$$

$$Eff. = \frac{25298}{225986} = \cdot 1119$$

Case 8.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad r = 1{\cdot}33$
$p_2 = 83{\cdot}77$ $\quad \tau_2 = 776{\cdot}2$
$p_3 = 4$

$$U_1 D_1 = 77{\cdot}2 \times \cdot 2627 \left[799 - 776{\cdot}2 \left(1 + hyp\ log \frac{799}{776{\cdot}2} \right) \right]$$

$$\frac{799 - 776{\cdot}2}{799} \times 177760 + 1{\cdot}33\ (83{\cdot}77 - 4)\ 144 = 20344$$

$$Eff. = \frac{20344}{225986} = \cdot 0898$$

Case 9.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad r = 1$
$p_2 = 115$ $\quad \tau_2 = 799$
$p_3 = 4$

$$U_1 D_1 = 772 \times \cdot 2627 \left[799 - 799 \left(1 + hyp\ log \frac{799}{799} \right) \right]$$

$$\frac{799 - 799}{799} + 1\ (115 - 4)\ 144 = 15984$$

$$Eff. = \frac{15984}{225986} = \cdot 0707$$

COMPUTATIONS—IDEAL CASES.

With Jacket.

Case 1.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad V_1 = 3{\cdot}806$ $\quad r = 20$
$p_2 = 5{\cdot}63$ $\quad \tau_2 = 628{\cdot}9$ $\quad V_2 = 76{\cdot}12$
$p_3 = 4$

$$U = a\ hyp\ log \frac{799}{628{\cdot}9} - b\ (799 - 628{\cdot}9) + 76{\cdot}12\ (5{\cdot}63 - 4)\ 144$$

$$= 191170$$

$$h = 772\,(628{\cdot}9 - 561{\cdot}2) + a\left(1 + hyp\ log\ \frac{799}{628{\cdot}9}\right) - b\ 799$$

$$= 99529$$

$$Eff.\ \frac{u}{h} = \frac{191170}{995290} = {\cdot}193$$

Case 2.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad V_1 = 3{\cdot}806$ $\quad r = 10$
$p_2 = 9$ $\quad \tau_2 = 649{\cdot}5$ $\quad V_2 = 38{\cdot}06$
$p_3 = 4$

$$U = a\ hyp\ log\ \frac{799}{649{\cdot}5} - b\,(799 - 649{\cdot}5) + 38{\cdot}06\,(9 - 4)\,144$$

$$h = 772\,(649{\cdot}5 - 561{\cdot}2) + a\left(1 + hyp\ log\ \frac{799}{649{\cdot}5}\right) - b.\ 799$$

$$= 975517$$

$$Eff. = \frac{1763{\cdot}02}{9755{\cdot}17} = {\cdot}1808$$

Case 3.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad v_1 = 3{\cdot}806$ $\quad r = 6{\cdot}66$
$p_2 = 15{\cdot}33$ $\quad \tau_2 = 649{\cdot}5$ $\quad v_2 = 25{\cdot}34$
$p_3 = 4$

$$U = a\ hyp\ log\ \frac{799}{675{\cdot}4} - b\,(799 - 675{\cdot}4) + 25{\cdot}34\,(15{\cdot}33 - 4)\,144$$

$$= 158179$$

$$h = 772\,(675{\cdot}4 - 561{\cdot}2) + a\left(1 + hyp\ log\ \frac{799}{675{\cdot}4}\right) - b.\ 799$$

$$= 949644$$

$$Eff. = \frac{158179}{949644} = {\cdot}1665$$

Case 4.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad V_1 = 3{\cdot}806$ $\quad r = 4$
$p_2 = 26{\cdot}36$ $\quad \tau_2 = 704{\cdot}2$ $\quad V_2 = 15{\cdot}22$
$p_3 = 4$

$$U = a\ hyp\ log\ \frac{799}{704{\cdot}2} - b\,(799 - 704{\cdot}2) + 15{\cdot}22\,(26{\cdot}36 - 4)\,144$$

$$= 133244$$

$$h = 772\,(704{\cdot}2 - 561{\cdot}2) + a\left(1 + hyp\ log\ \frac{799}{704{\cdot}2}\right) - b.\ 799$$
$$= 923856$$
$$Eff. = \frac{133244}{923856} = {\cdot}1442$$

Case 5.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad V_1 = 3{\cdot}806$ $\quad r = 2{\cdot}857$
$p_2 = 37{\cdot}69$ $\quad \tau_2 = 724{\cdot}7$ $\quad V_2 = 10{\cdot}87$
$p_3 = {\cdot}4$

$$U = a\ log\ \frac{799}{724{\cdot}7} - 540{\cdot}4\,(799 - 724{\cdot}7)$$
$$+ 10{\cdot}87\,(37{\cdot}69 - 4)\,144 = 118321$$
$$h = 772\,(724{\cdot}7 - 561{\cdot}2) + a\left(1 + hyp\ log\ \frac{799}{724{\cdot}9}\right) - b.\,(799)$$
$$= 909723$$
$$Eff. = \frac{118321}{909723}\ {\cdot}2301$$

Case 6.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad V_1 = 3{\cdot}08$ $\quad r = 2{\cdot}22$
$p_2 = 49{\cdot}28$ $\quad \tau_2 = 740{\cdot}8$ $\quad V_2 = 8{\cdot}449$
$p_3 = 4$

$$U = a\ hyp\ log\ \frac{799}{740{\cdot}8} - b\,(799 - 740{\cdot}8) + 8{\cdot}449\,(49{\cdot}28 - 4)\,144$$
$$= 108963$$
$$h = 772\,(740{\cdot}8 - 561{\cdot}2) + a\left(1 + hyp\ log\ \frac{799}{740{\cdot}8}\right) - b\ (799)$$
$$= 901746$$
$$Eff. = \frac{108963}{901746} = {\cdot}1208$$

Case 7.

$p_1 = 115$ $\quad \tau_1 = 799$ $\quad V_1 = 3{\cdot}806$ $\quad r = 1{\cdot}818$
$p_2 = 60{\cdot}93$ $\quad \tau_2 = 753{\cdot}7$ $\quad V_2 = 6{\cdot}919$
$p_3 = 4$

$$U = a\ hyp\ log\ \frac{799}{753{\cdot}7} - b\,(799 - 753{\cdot}7) + 6{\cdot}919\,(60{\cdot}93 - 4)\,144$$
$$= 96883$$

$$h = 772 \times (753 - 561) + a\left(1 + hyp\ log \frac{799}{753}\right) - b\ 799$$
$$= 890956$$
$$Eff. = \frac{96883}{890956} = \cdot 1087$$

Case 8.

$p_1 = 115$ $\tau_1 = 799$ $V_1 = 3{\cdot}806$ $r = 1{\cdot}33$
$p_2 = 85$ $\tau_2 = 777{\cdot}2$ $V_2 = 5{\cdot}161$
$p_3 = 4$

$$U = a\ hyp\ log \frac{799}{777{\cdot}2} - b\,(799 - 777{\cdot}2) + 5{\cdot}161\,(8{\cdot}5 - 4)\,144$$
$$= 70387$$
$$h = 772\,(777{\cdot}2 - 561{\cdot}2) + a\left(1 + hyp\ log \frac{799}{777{\cdot}2}\right) - b.\,799$$
$$= 86649$$
$$Eff. = \frac{70387}{866490} = \cdot 0812$$

Case 9.

$p_1 = 115$ $\tau_1 = 799$ $V_1 = 3{\cdot}806$ $r = 1$
$p_2 = 115$ $\tau_2 = 799$ $V_2 = 3{\cdot}806$
$p_3 = 4$

$$U = a\ hyp\ log \frac{799}{799} - b\,(799 - 799 + 3{\cdot}806\,(115 - 4)\,144$$
$$h = 772\,(799 - 561{\cdot}2) + a\left(1 + hyp\ log \frac{799}{799}\right) - b.\,799$$
$$= 861350$$
$$Eff. = \frac{60835}{861350} = \cdot 0707$$

Tabulating the results as obtained under the several cases, we have

EFFICIENCIES OF WORKING FLUID.

Steam engine, Jacketed and Unjacketed.

Cut-off.	*Ratio Exp.*	*Eff. Without.*	*Eff. with Jacket.*
0·05	20·00	0.2073	0·1930
·10	10·00	·1934	·1808
·15	6·66	·1795	·1665
·25	4·00	·1566	·1442
·35	2·85	·1358	·1302
·45	2·22	·1237	·1209
·55	1·82	·1119	·1087
·75	1·33	·0898	·0812
1·00	1·00	·0707	·0707

An examination of the tables, of the curves still better, will show clearly the wasteful influence of the steam jacket, as an element considered by itself. Within the usual range of practice, from about five or six to fifteen or twenty expansions, under the assumed conditions of initial pressure and cut-off, it is seen that the loss by its application is fairly constant at something above one per cent., in these cases; rapidly falling to zero as the ratio of expansion falls from the lower figures to unity. The consumption of steam in pounds per horse-power per hour, may be computed very approximately by dividing 2·5 by the computed efficiencies. The cases assumed are for condensing engines, and the evaporation always taken at nine pounds of steam per pound of fuel, the fuel expenditure may be gauged by dividing the weight of steam computed by nine. This gives, for example, about 12·06 and 12·95 pounds for the unjacketed and for the jacketed engine, respectively, at a ratio of twenty, in steam demanded; and of about 1·33 and 1·44 pounds of fuel. For a ratio of expansion of four, the figures become about 16 and 17·3 respectively, for the steam and 1·75 and 1·85 pounds of fuel. At full stroke, the figures become 35 pounds of steam and of feed water, and 4 pounds of fuel per horse-power and per hour, for both engines.

We conclude, from the above, therefore, that, in the ideal case, the steam jacket reduces efficiency, necessarily and without exception, and that, for ordinary variations of the ratio of expansion for the pressure here assumed, this waste is a nearly constant fraction. To put it more mathematically, the gain is negative, and nearly constant at about one per cent. efficiency.

(2) REAL EFFICIENCIES.—In 1886, a "Research Committee" was appointed by the British Institution of Mechanical Engineers, to investigate the subject of the steam jacket.* A very unusually complete set of data, pertaining to trials made with a view to determine the efficiency produced by application of the jacket was secured, and from these the following figures were collated and results deduced:

* Proceedings, 1890.

(1) The case of a single-cylinder non-condensing Corliss engine, 21·65 x 43·31 inches, the body only jacketed. The jackets were supplied by a small pipe from the main steam pipe and were automatically drained.

Following are the data as obtained, the experiments being carried on at variable boiler pressures and ratios of expansion. The comparisons to be given, like the preceding, were made, and reported to the writer, by Messrs. Hitchcock and Mount.

STEAM-JACKET EFFICIENCIES.

(See Curves 1, Set I, Fig. II.)

	Without.	*With.*
Jackets,	—	—
Boiler pressure above atmos.,	110	110
Expansions,	6·2	7·1
Piston speed,	445	439
Feed water per I. H. P. per hr.,	28·38	22·80
Jackets,	—	—
Boiler pressure,	110	110
Expansions,	6·2	6·2
Piston speed,	445	447·5
Feed water,	28·38	22·13
Jackets,	—	—
Boiler pressure,	110	110
Expansions,	5	5·3
Piston speed,	443	447·5
Feed water,	26·82	22·35
Jackets,	—	—
Boiler pressure,	110	110
Expansions,	4·4	4·4
Piston speed,	459	453
Feed water,	27·58	23·65

(Curves 2, Set I, Fig. II.)

Jackets,	78.2	7
Boiler pressure,	78·2	78·2
Expansions,	5·4	5·1
Piston speed,	447	441
Feed water,	27·58	23·65
Jackets,	—	—
Boiler pressure,	78·2	78·2
Expansions,	4·8	5·1

STEAM-JACKET EFFICIENCIES.—(*Continued.*)

	Without.	*With.*
Piston speed,	440	441
Feed water,	26·71	23·65
Jackets,	—	—
Boiler pressure,	78·2	78·2
Expansions,	3·7	3·8
Piston speed,	433	445
Feed water,	24·58	22·58
Jackets,	—	—
Boiler pressure,	78·2	78·2
Expansions,	2·9	3·1
Piston speed,	437	437
Feed water,	24·18	21·95
(*See Curves 3, Set I, Fig. II.*)		
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	3·7	3·9
Piston speed,	443	437
Feed water,	27·27	25·3
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	2·5	2·8
Piston speed,	441	437
Feed water,	27·2	25·21
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	1·7	1·7
Piston speed,	440	440
Feed water,	30·17	28·74
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	1·	1·
Piston speed,	437	441
Feed water,	46·82	46·26

Curves *A B C* on *Fig. IV*, are derived from curves *1*, *2*, *3*, *Set I* on *Fig. II*, by determining the efficiency of the jacket for different ratios of expansion in the following manner: A certain ratio of expansion was taken and, by comparing the amounts of feed water, as read from the curves, for that

ratio, we determine the difference in efficiency due to the jacket for that expansion.

These curves show an increased efficiency with increase in ratio of expansion.

The second case is that of a single cylinder condensing engine (Corliss), cylinder dimensions same as before, body only jacketed; experiments carried on at the same place, in the same year and in the same manner.

The following are the data as obtained:

STEAM-JACKET EFFICIENCIES.

(*See Curves 1, Set II, Fig. II.*)

	Without.	*With.*
Jackets,	—	—
Boiler pressure,	110	110
Expansions,	13·5	11·3
Piston speed,	433	424
Feed water,	23·24	17·05
Feed water ideal case,	15·63	17·00
Jackets,	—	—
Boiler pressure,	110	110
Expansions,	10·7	10
Piston speed,	423	444
Feed water,	22·24	16·65
Feed water ideal,	17·00	17·30
Jackets,	—	—
Boiler pressure,	110	110
Expansions,	8·2	10
Piston speed,	429	432
Feed water,	21·41	16·50
Feed water ideal,	17·70	17·30
Jackets,	—	—
Boiler pressure,	110	110
Expansions,	6·4	6·4
Piston speed,	416	419
Feed water,	21·99	17·59
Feed water ideal,	17·60	18·94

(*See Curves 2, Set II, Fig. II.*)

Jackets,	—	—
Boiler pressure,	88·9	88·9
Expansions,	9·3	12
Piston speed,	432	430
Feed water,	21·19	17·66

STEAM-JACKET EFFICIENCIES.—(*Continued.*)

	Without.	*With.*
Jackets,	—	—
Boiler pressure,	88·9	88·9
Expansions,	8·7	11·3
Piston speed,	428	430
Feed water,	21·05	17·32
Jackets,	—	—
Boiler pressure,	88·9	88·9
Expansions,	7·3	6·8
Piston speed,	432	429
Feed water,	20·79	16·87
Jackets,	—	—
Boiler pressure,	88·9	88·9
Expansions,	5·9	5·9
Piston speed,	419	419
Feed water,	19·89	17·5
Jackets,	—	—
Boiler pressure,	88·9	88·9
Expansions,	4·8	5·9
Piston speed,	427	433
Feed water,	20·43	17·50
(*See Curves 3, Fig. III; Set II, Fig. II.*)		
Jackets,	—	—
Boiler pressure,	64	64
Expansions,	10·8	10·7
Piston speed,	4·21	4·32
Feed water,	20·41	18·48
Jackets,	—	—
Boiler pressure,	64	64
Expansions,	8·2	8·2
Piston speed,	429	430
Feed water,	19·09	17·59
Jackets,	—	—
Boiler pressure,	64	64
Expansions,	5·4	5·4
Piston speed,	426	424
Feed water,	18·06	17·34
Jackets,	—	—
Boiler pressure,	64	64
Expansions,	3·9	4·4
Piston speed,	421	427
Feed water,	18·37	17·66

STEAM-JACKET EFFICIENCIES.—(*Continued.*)

	Without.	*With.*
Jackets,	64	64
Boiler pressure,	64	64
Expansions,	3·6	3·6
Piston speed,	427	426
Feed water,	18·84	18·55
(*See Curves 4, Set II, Fig. II.*)		
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	10·7	12
Piston speed,	438	435
Feed water,	20·65	19·29
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	5·6	5·6
Piston speed,	436	431
Feed water,	18·77	18·17
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	7·9	7·6
Piston speed,	424	416
Feed water,	19·36	18·48
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	4·2	4
Piston speed,	424	434
Feed water,	19	18·91
Jackets,	—	—
Boiler pressure,	49·8	49·8
Expansions,	3·2	3·2
Piston speed,	424	432
Feed water,	19·78	19·67

Curves constructed as before are here marked *D E F* and *G* on *Fig. IV;* being derived from *1*, *2*, *3* and *4*, Set II. *Fig. II.* These curves also show that the efficiency of the jacket with such engines increases with increase in ratio of expansion, and in the case of *D* and *E* increases up to a certain value of the ratio of expansion and then decreases.

The third case applies to a horizontal compound con-

densing tandem engine, the body of the cylinders only being jacketed. The whole steam supply to the cylinders passed through the jackets, which were drained by trap; and when not in use the jackets were open to the air.

The trials were carried on at a constant boiler pressure of forty-two pounds above atmosphere and a piston speed of 196 feet per minute.

STEAM-JACKET EFFICIENCIES.

Without Steam in either Jacket.

(*See Curves 1, Fig. III.*)

Expansions,		8·38	7·54
Thermal units rejected per I. H. P. per m.,		5·25	5·33
Expansions,		5·8	4·45
Thermal units rejected per I. H. P. per m.,		619	574

With Steam in High-pressure Jacket Alone.

(*See Curves 2, Fig. III.*)

Expansions,	10·78	7·73	7·01
Thermal units rejected,	420	420	418

With Steam in Low-pressure Jacket Alone.

(*See Curves 3, Fig. III.*)

Expansions,	15·8	9·74	9·14
Thermal units rejected,	427	380	388
Expansions,		8·38	7·54
Thermal units rejected,		383	385
Expansions,		7·18	5·38
Thermal units rejected,		407	427

With Steam in both Jackets.

(*See Curves 4, Fig. III.*)

Expansions,	13·1	13·1	8·38
Thermal units rejected per I.H.P. per m.,	347	348	375
Expansions,	9·14	7·54	6·7
Thermal units rejected,	367	372	385
Expansions,		5·8	4·64
Thermal units rejected,		370	410

COMPUTED DATA, BY WHICH CURVES ON FIG. IV ARE PLOTTED.

Curve.	Ratio of exp.	Eff. of jacket. Per cent.	Curve.	Ratio of exp.	Eff. of jacket. Per cent.
A	6·2	21·33	F	8	7·38
	6	20·61		7	5·94
	5	15·79		6	4·67
B	5·5	13·6		5	3·6
	5	12		4	1·64
	4	9·63	G	10	7·45
	3	9·38		9	6 28
C	4	7·64		8	5·13
	3	7·		7	3·84
	2	3·93		6	3·08
D	12	23·43		5	2·03
	11	23·82	H	9	19·23
	10	23·78		8	20·45
	9	23·2		7	22·55
	8	21·5	I	9	26·34
	7	20·32		8	26·34
E	10	21·95		7	26·24
	9	22·14		6	26·5
	8	19·23	K	9	29·
	7	16·66		8	29·35
	6	12·34		7	30·
F	10	9·25		6	31·62
	9	8·62			

The preceding work, including the computations and all the comparisons of data, has been performed with great care, the two computers checking the figures and the results, in such manner that, as shown by the forms of the curves, there can be no doubt of the existence of this maximum in the value of the steam jacket, the ratios of expansion being varied as seen, and it is probably fairly to be assumed that it may be found in all cases. These data and deductions will well repay a much more extended study than it is here practicable to give them. A short review of the work will, however, reveal some interesting and probably important facts.

In the first case, that of the simple non-condensing Corliss engine, the heads unjacketed, we see, taking the first example, that the use of the jacket reduced the cylinder-wastes from about twenty-five per cent. of the ideal consumption of steam and feed water to about half that pro-

portion, for ratios of expansion approximating six; from one-third to about one-tenth, at a ratio of five; and apparently from twenty to ten per cent. at 4·4. The same general effect is observed throughout, with some discrepancies which may be due either to varying action of the jacket or to slight errors of observation, or to both combined, the latter being the probable fact.

In this first case, also, it will be observed that the jacket gives best results, with 110 pounds of steam, when the ratio of expansion approximates six. When the steam pressure falls to approximately eighty pounds, the best work of the jacket occurs at a ratio not far from 4·75; while, at the pressure of fifty pounds, the value of the jacket increases through the whole range of the experiments, and not only so, but the curve assumes a rectilinear form, indicative of probable improvement indefinitely in the direction of increasing expansion. The highest efficiencies, however, either with or without the jacket are found, in this case, at the lowest ratios adopted, and indicate a maximum value at about 3·25. The ratios of expansion for maximum efficiency of fluid, in the other cases, are seen to be, for 110 pounds, about five, and for eighty pounds, about 3·5.

Similarly studying the performance of the condensing engine, we find that the best work done, whether jacketed or not, at about a ratio of expansion of ten (at a steam-pressure of 110 pounds), but that the jacketed engine reduces the internal wastes from fifty per cent. at highest ratios, and from one-fourth at the lowest ratios, in the case of the unjacketed engine, to five per cent., and, in some cases, probably to within the magnitude of the errors of observation. At a pressure of ninety pounds the best ratio seems to be for this engine, under the given conditions of operation, about 6·5 when unjacketed, and 8·5 jacketed; while the lower pressures still further reduce both the efficiencies and the savings effected by the jacket. The best work of the jacket, as an economizer of heat, is done at high pressure, at a ratio of expansion of twelve or more. In all cases it seems to be the fact, with these engines at least, that the jacket is useful beyond the ratios of maximum efficiency of fluid.

The compound engine is operated at altogether too low a pressure to bring out the best effect of compounding; but it exhibits the same general effects which have been noted in the cases of working of simple engines. The effect of the jacket is less pronounced than in the simple engine, and the efficiencies of fluid vary less with variation of the ratios of expansion. It gives its best result at ratios of expansion ranging from 7·5 to 10·5, the variations of the value being very much more observable in the last case, in which both jackets are in use, than in either of the others, and least in Case 3, in which only the high-pressure jacket was employed. By far the best work was done by the engine when both jackets were in use.

This discovery of a maximum efficiency of jacket may throw some light upon the causes of the conflicting and sometimes apparently irreconcilable results of trials of engines with and without jackets, and with jackets variously constructed. The discovery may also prove of value to the designer, as aiding him in securing the best proportions and arrangement of his engine.

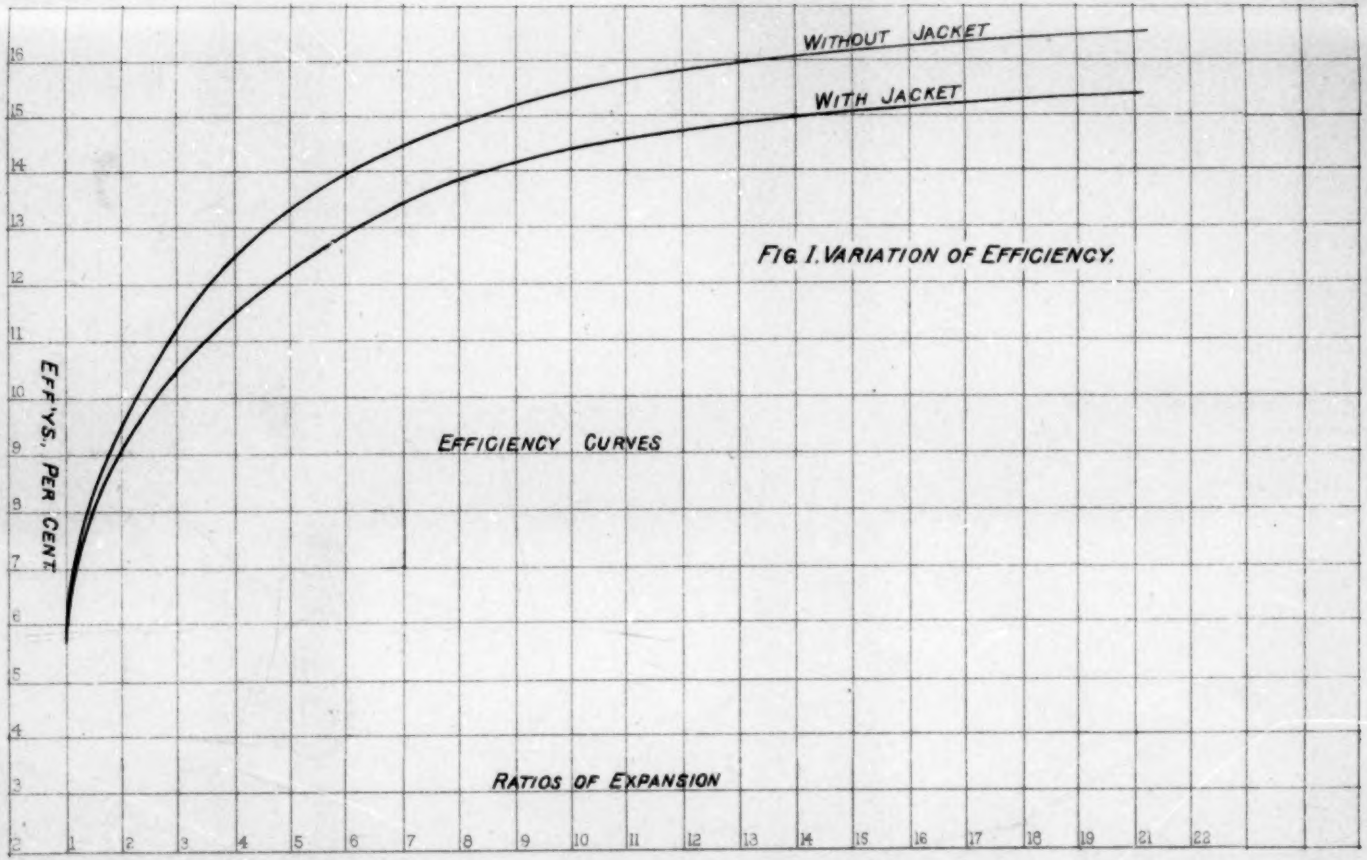

FIG. I. VARIATION OF EFFICIENCY.

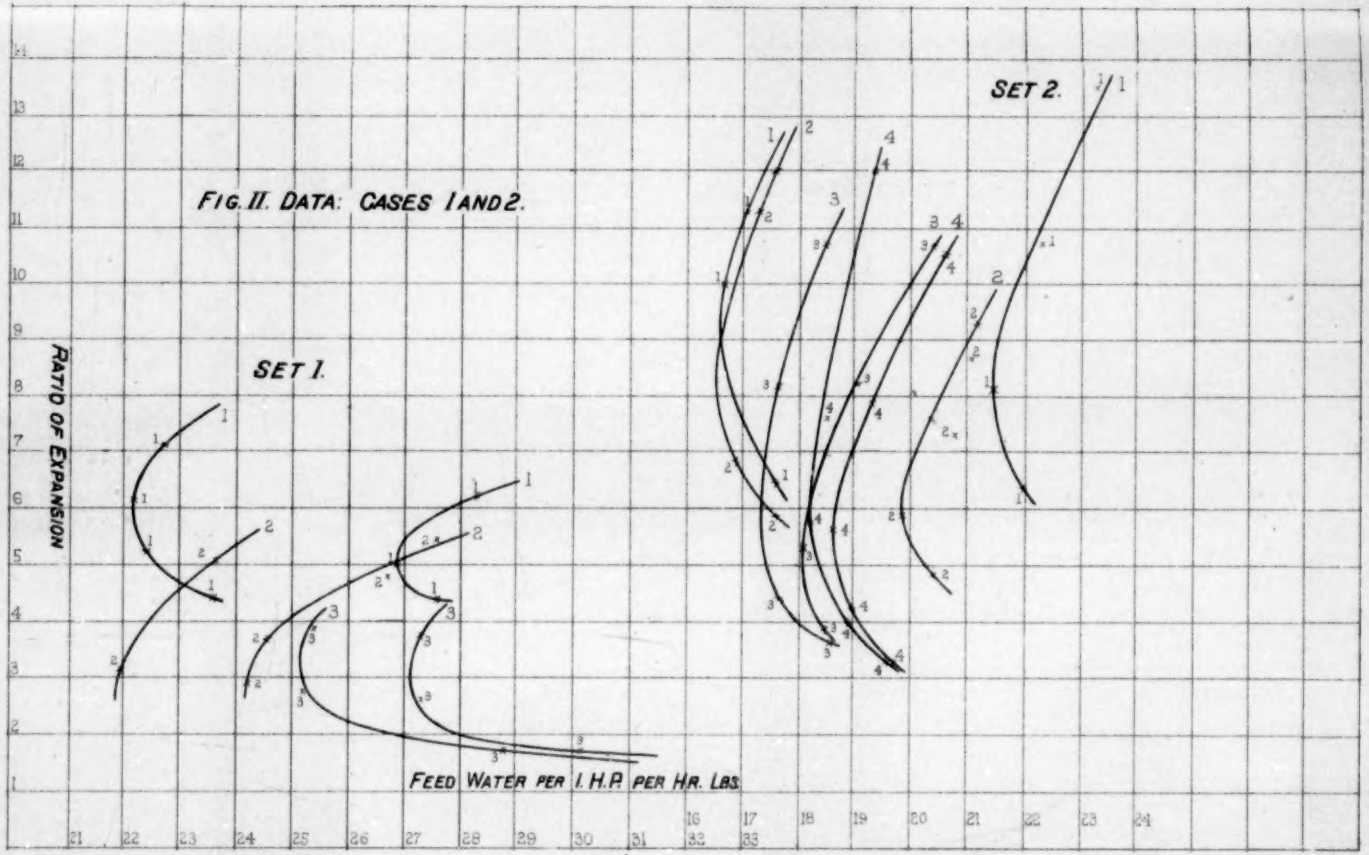

FIG. II. DATA: CASES 1 AND 2.

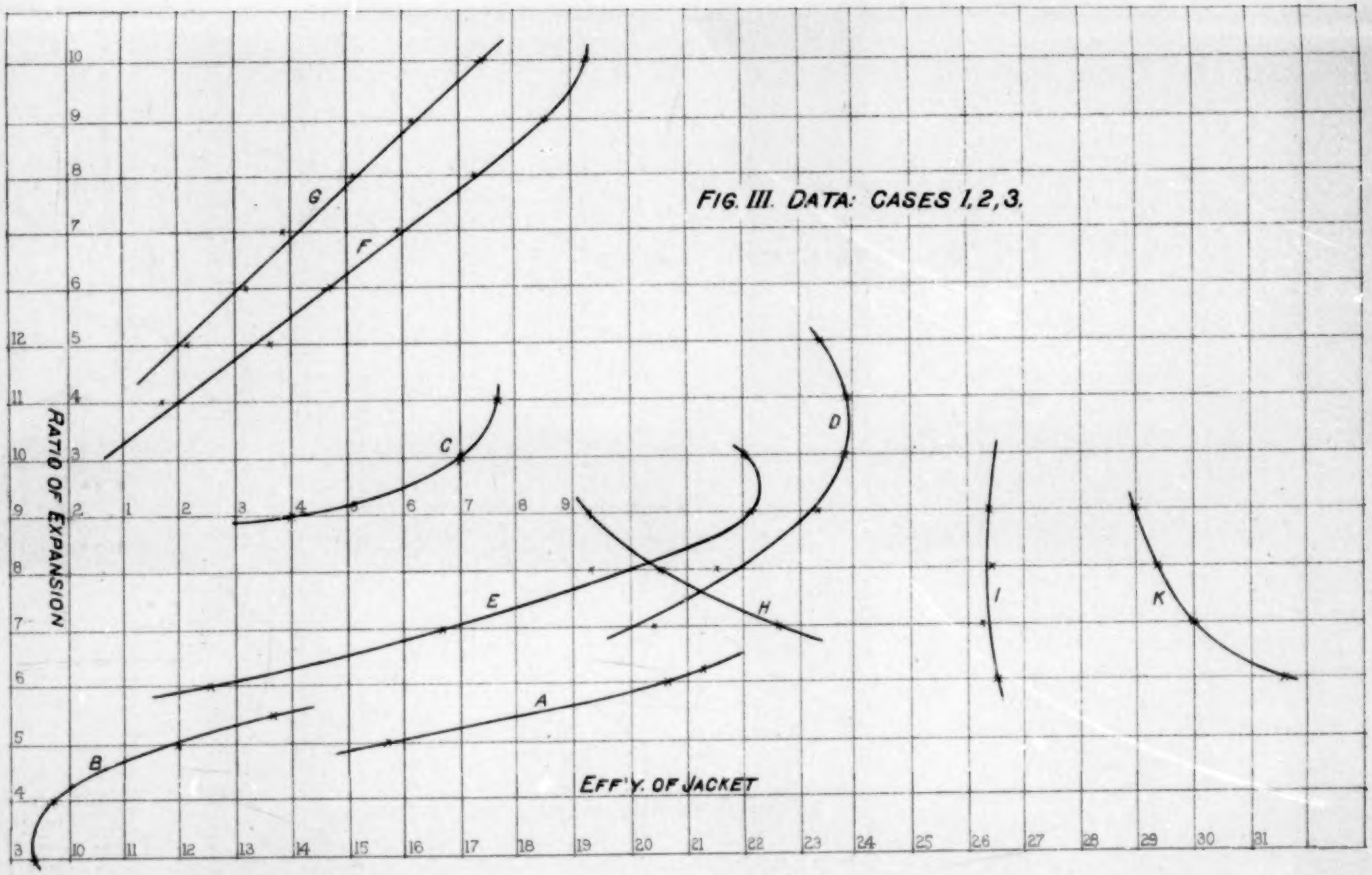

FIG. III. DATA: CASES 1, 2, 3.

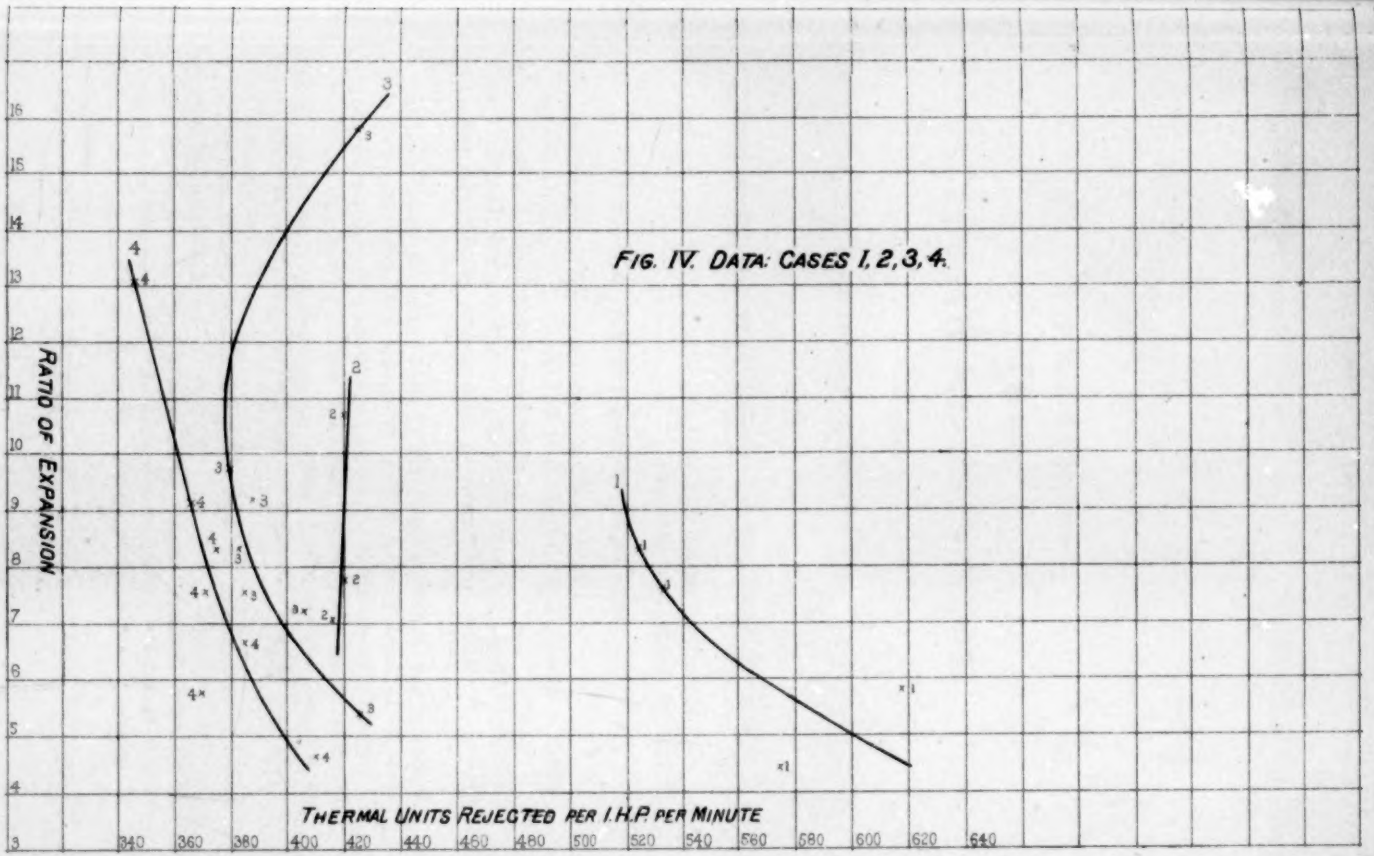

FIG. IV. DATA: CASES 1, 2, 3, 4.

PROCEEDINGS

OF THE

CHEMICAL SECTION

OF THE

FRANKLIN INSTITUTE.

[*Stated meeting held at the institute, Tuesday, March 17, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, March 17, 1891.

Dr. WM. H. WAHL, president, in the chair.

There were present fifteen members and four visitors.

Letters from Dr. F. A. Geuth and Prof. Joseph W. Richards, acknowledging election to membership, were read.

Mr. George H. Bartram read an interesting paper on "A source of error in the phenol-sulphuric acid method of determining nitrates in potable waters."

The paper was commented upon by Mr. Reuben Haines, who remarked that in his experience in the use of this method he had suspected the existence of such a source of error as was pointed out in Mr. Bartram's paper.

Mr. Fred. E. Ives read a paper criticising adversely the results obtained by Prof. Lippmann in photographing colors. The paper was discussed by Messrs. Hall, Wahl, Jayne and Haines; considerable interest was manifested in the paper.

Mr. Reuben Haines then read a paper, entitled "Percentage of iodine absorbed by lard oil."

Mr. H. Pemberton, Jr., presented two papers, entitled, respectively, "Analysis of a chromite" and "An apparatus for heating substances in glass tubes;" they were read by title. The papers presented were referred for publication.

Adjourned.

WM. C. DAY, *Secretary.*

THE ELECTROLYTIC METHOD APPLIED TO RHODIUM.

BY EDGAR F. SMITH.

[*Read at the stated meeting, held February 17, 1891.*]

So far as I have been able to learn, the salts of rhodium have never been subjected to the action of the electric current. I, therefore, made experiments in this direction and obtained results indicating the possibility of employing the electrolytic method in the estimation of this metal. The various texts consulted led me to believe that in all likelihood a solution of the double cyanide of rhodium and an alkali metal would best answer my purposes. Precipitation did not occur upon mixing solutions of potassium cyanide and rhodio-sodium chloride. The liquid remained clear. As soon, however, as a comparatively feeble current acted upon the double cyanide, a reddish yellow colored compound appeared, and remained suspended in the liquid. It was probably a cyanide which sustained no change. I did not attempt a repetition of this experiment.

From my experience in the electrolysis of gold, platinum and palladium solutions containing alkaline phosphates and free phosphoric acid, I concluded to try rhodium under similar conditions. Sodium rhodium chloride, $Na_6Rh_2Cl_6 + 24H_2O$, was the salt used by me. A sufficient quantity of it was dissolved so that 10 cc. of liquid contained, by calculation, 0·0980 gram of metallic rhodium.

Experiments.—(1) To 10 cc. of the rhodium solution were added 30 cc. of sodium phosphate, Na_2HPO_4 (sp. gr. 1·0358), and 3 cc. of phosphoric acid (sp. gr. 1·347). The total dilution of the electrolyte was 180 cc. The current gave 1·8 cc. OH gas per minute. In seven hours the deposition of metal was finished.

Weight of crucible + rhodium	= 63·4765
" " " alone,	= 63·3783
Rhodium =	0·0982

The deposition of metal occurred at the ordinary temperature. At the beginning of the decomposition the liquid showed a beautiful deep purple color, but as the metal separated, it rapidly became lighter and finally colorless. Upon inclining the dish in which the precipitation was made, thus exposing a fresh metal surface, the latter remained perfectly clear. The precipitation of metal was completed.

(2) The conditions here were nearly the same as in the preceding trial. The total dilution of liquid equalled 200 cc. while the current gave 1·6 cc. of OH gas per minute.

The metallic rhodium was precipitated upon copper-plated platinum dishes. It was rather black in color, very compact and perfectly adherent. It was washed without any difficulty. Hot water was used for this purpose. The drying was done upon a warm iron plate.

The results obtained accord so well with the theory that the accuracy of the method cannot be questioned. The rapidity with which the metal is deposited, and the ease with which it may be handled also recommend this method of estimation.

CHEMICAL LABORATORY OF THE UNIV. OF PA.,
PHILADELPHIA, February 13, 1891.

THE ELECTROLYTIC DETERMINATION OF MERCURY.

BY EDGAR F. SMITH.

[*Read at the stated meeting, held February 17, 1891.*]

In gravimetric analysis mercury is frequently obtained as sulphide. To weigh it as such, or to convert it into some form suitable for weighing, requires much time and close attention to insure results that will be at all satisfactory. It is, therefore, better to have recourse to the electrolytic method of determination. As mercury sulphide dissolves quite readily in the fixed alkaline sulphides, I electrolyzed such solutions. The results show that this

procedure affords a very accurate and clean method for the estimation of this metal.

The sodium sulphide was prepared as described in the paper relating to the determination of gold. Its specific gravity was 1·19.

Solutions of mercuric chloride, containing a known amount of metal, were first tried. The table shows the conditions of experiment and the results:

Mercury present in grams.	Sodium sulphide present. Sp. gr. 1·19.	Total dilution.	Current in cc. OH gas per minute.	Mercury found.
0·1903	20 cc.	125 cc.	1 cc.	0·1902
0·1903	20 cc.	125 cc.	1 cc.	0·1905
0·1903	20 cc.	125 cc.	1 cc.	0·1907
0·1903	20 cc.	130 cc.	1·2 cc.	0·1910
0·1903	30 cc.	130 cc.	1·2 cc.	0·1900

The precipitation extended through the night. The deposited mercury was gray in color and very compact. Only once was a tendency to the fluid form observed. Hot water was used for washing. The metal was dried by holding the platinum dish in the palm of the hand and blowing gently upon the deposit.

If mercury and arsenic are both present in a solution of sodium sulphide, the current will throw out the former. It will not carry down any arsenic. This is evident from the two analyses that follow:

Mercury present.	Arsenic present.	Sodium sulphide.	Total dilution.	Current in cc. OH gas per minute.	Mercury found.
0·1903 gr.	100 %	25 cc.	125 cc.	2 cc.	0·1908 gr.
0·1903 gr.	100 %	25 cc.	125 cc.	2 cc.	0·1894 gr.

In the communication upon the deposition of gold from sodium sulphide solutions, mention is made that gold and tin could not be separated electrolytically when present together in a solution of that kind. I was, therefore, rather surprised to discover that mercury could be separated from

tin under such circumstances. The conditions of experiment were similar to the following:

The solution contained 0·1903 gram of mercury, 0·1200 gram of tin, and 30 cc. of sodium sulphide. The total dilution was 125 cc. The current gave 2 cc. of OH gas per minute, and acted for twelve hours. The precipitated mercury weighed 0·1909 gram.

The filtrates from the mercury deposits did not show even traces of this metal when they were examined.

CHEMICAL LABORATORY OF THE UNIV. OF PA.,
PHILADELPHIA, February 13, 1891.

THE ELECTROLYTIC DETERMINATION OF GOLD.

BY EDGAR F. SMITH.

[*Read at the stated meeting, held February 17, 1891.*]

Gold has been estimated electrolytically in solutions, containing it as a double cyanide, or in the presence of sodium phosphate and phosphoric acid. From recent experiments I am satisfied it can be determined in the same manner by the decomposition of solutions of sodium sulphaurate.

Years ago Parodi and Mascazzini (*Zeit. f. Analyt. Chemie*, **18**, 588), showed that antimony was completely precipitated from solutions of its ammonium sulpho-salt by the electric current. Classen elaborated this idea (*Ber.* **14**, 1622; **17**, 2467; **17**, 2245; **18**, 1110), substituting sodium sulphide for ammonium sulphide, and succeeded in effecting a most excellent separation of antimony from both tin and arsenic.

The sodium sulphide, used in the experiments recorded in this communication, was prepared by supersaturating a definite volume of caustic soda (1·3 sp. gr.) with hydrogen sulphide gas. An equal amount of caustic soda was then added, and the current of hydrogen sulphide continued for some hours through the solution. The liquid was then rapidly concentrated, until a crystalline scum appeared upon its surface. In this condition it was placed in bottles

provided with tight-fitting corks. Crystals separated as the liquid cooled. These were recrystallized, and then dissolved for use. The specific gravity of their solution equalled 1·1800.

Experiments.—(1) To a chloride solution of gold containing 0·1446 gram of metallic gold, were added 10 cc. of sodium sulphide (Na_2S), and 100 cc. of water. The electric current, allowed to act upon this mixture, gave 2·6 cc. OH gas per minute. The time of precipitation extended through the night.

The deposit of metallic gold weighed 0·1446 gram. It was bright yellow in color, and very adherent. Water alone was used to wash it. The drying was done upon a warm iron plate.

(2) In this experiment the same quantity of gold was present as in the first trial. The volume of sodium sulphide did not exceed 20 cc., while the total dilution of the electrolyte was 100 cc. The current gave 2·6 cc. OH gas per minute. The metallic deposit was bright in appearance, firm and adherent. It weighed 0·1446 gram. The precipitation was complete in five and one-half hours.

The depositions of metal were made directly upon platinum. The gold was removed by covering it with a dilute solution of potassium cyanide, allowing the latter to remain for several hours in the dishes. When desired, the gold deposit can be very speedily removed by connecting the dish with the anode of a battery, and employing a platinum rod, which dips into the solution, as cathode. Platinum will not dissolve if feeble currents are used.

The results, given above, leave no doubt as to the applicability of this method in the estimation of gold.

Subsequent efforts were directed to its separation from other metals. As antimony can be deposited from a sodium sulphide solution by a current giving 1·5–2·0 cc. of OH gas per minute, I made no attempt to separate it from gold. The separation of gold from tin was much desired. Numerous trials were made. The results, however, were unsatisfactory. The gold was, in all instances, precipitated, but it carried down with it from one to three per cent. of tin.

The sodium sulphide, in these separations, showed a specific gravity of 1·21. An additional gram of sodium hydroxide was introduced into the electrolyte for every centigram of metallic tin present. The current did not exceed 1·5 cc. of OH gas per minute. The dilution with water was 100 cc. While these conditions were exactly analogous to those in which antimony is separated from tin, they failed in the case of gold and tin.

Gold from Arsenic.—(1) The solution electrolyzed contained 0·1446 gram of gold, as chloride, 20 cc. of sodium arsenate solution (= 0·15920 gr. metallic arsenic), 60 cc. sodium sulphide, and 100 cc. of water. The current gave 1·2 cc. OH gas per minute. It ran through the night. The washed and dried deposit of gold weighed 0·1443 gram.

(2) With conditions similar to those in (1) the gold weighed 0·1446 gram.

Gold from Molybdenum.—(1) 0·1437 gram of gold, as chloride, 0·1500 gram molybdenum, as ammonium molybdate, 40 cc. of sodium sulphide, and 120 cc. water were exposed to the influence of a current giving 0·7 cc. OH gas per minute. The time of deposition was twelve hours. The gold weighed 0·1430 gram.

(2) 0·1178 gram of gold, as chloride, 0·1200 gr. of molybdenum, 30 cc. of sodium sulphide (sp. gr. 1·12), and 100 cc. of water were electrolyzed with a current giving 2 cc. OH gas per minute.

The precipitated gold weighed 0·1172 gram. The current acted ten hours.

Gold from Tungsten.—(1) 0·1437 gram of metallic gold, as chloride, 0·1500 gram of tungsten, as ammonium tungstate, 20 cc. sodium sulphide (sp. gr. 1·17), and 100 cc. of water, gave 0·1440 gram gold, when electrolyzed with a current generating 1 cc. of OH gas per minute. The precipitation required twelve hours.

(2) In this trial 0·1440 gram of gold was found. The conditions were analogous to those of (1).

The gold deposits in all of the separations were bright and compact. The same manner of washing and drying was pursued as in the first determination of the metal.

Gold was never discovered in the liquid removed from the deposit.

Qualitative trials proved that the separation of gold from vanadium, under conditions similar to those already mentioned, was also possible.

CHEMICAL LABORATORY OF THE UNIV. OF PA.,
PHILADELPHIA, February 13, 1891.

PROCEEDINGS
OF THE
ELECTRICAL SECTION,
OF THE
FRANKLIN INSTITUTE.

[*Proceedings of the first stated meeting of the Electrical Section, held Tuesday, February 24, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, February 24, 1891.

Prof. EDWIN J. HOUSTON, in the chair.

The stated meeting of the Section was held this evening, at 8 o'clock.

There were present: Messrs. Billberg, Davids, C. Hering, H. Hering Houston, Hoadley, McDevitt, Pemberton, Pike, Rondinella, Salom, Spangler Wahl, Winand, and fifteen visitors.

The minutes of the special meeting for organization, held Saturday, January 31st, were read and approved.

The following communications were presented:

By Mr. Wm. McDevitt—On some dangers of electric lighting. Discussed by Messrs. C. Hering, Houston, Billberg, Salom, and the author.

By Mr. C. Hering—On a new standard cell. Discussed by Messrs. Hoadley, Perkins, Pike, Houston, H. Hering, and the author.

By Mr. C. W. Pike—On a new accumulator. Discussed by Messrs. C. Hering, Salom, and the author.

By Prof. H. Hering—On the effect of atmospheric pressure on batteries. Discussed by Messrs. Pemberton, Houston, C. Hering, Wahl, Spangler, Pike, and the author.

On motion of Prof. Rondinella, the remaining papers on the programme were postponed until the next stated meeting, in order to permit of the transaction of routine business.

The consideration of the by-laws, provisionally adopted at the special meeting of January 31st, was first taken up. The secretary read the regulations, which were thereupon considered section by section, and after suffering a number of amendments, were adopted as a whole.

Nominations for officers were then made, as follows:

For president—Prof. Edwin J. Houston.

" vice-presidents—Messrs. C. Hering and Pedro G. Salom.

" secretary—Prof. L. F. Rondinella.

" treasurer—Dr. Wm. H. Wahl.

" conservator—Dr. Wm. H. Wahl.

The secretary *pro tem.*, was directed to cast the ballot of the meeting for the foregoing nominees, which accordingly was done, and the chairman *pro tem.*, declared them to be the officers of the section for the current year.

Mr. C. Hering moved a suspension of the rules, in order that the next stated meeting might be somewhat deferred. Carried.

The same member moved that the next stated meeting be held on Tuesday, March 17th. Carried.

The president thereupon named the following members to serve upon the committee on admission of members, viz: Messrs. C. Hering, H. Hering, Billberg, Pike, Salom, Rondinella and Wahl.

Adjourned.

WM. H. WAHL,
Secretary pro tem.

[*Proceedings of the stated meeting, held March 10, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, March 10, 1891.

Prof. EDWIN J. HOUSTON in the chair.

Present: Twenty-three members and visitors.

The minutes of the previous meeting were read and approved.

The treasurer reported that copies of the section's by-laws had been printed, and that the necessary stationery and supplies had been obtained for the secretary and treasurer.

The committee on admissions reported the following elections: To regular membership, Messrs. John Hoskin, T. Carpenter Smith, David Pepper, Jr., H. W. Bartol, F. J. Firth, Craig R. Arnold, Fred'k H. Colvin and Alex. J. Whittingham; and to associate membership, Messrs. D. S. Rittenhouse, Wm. McClellan and H. Allen Higgins.

Mr. R. W. Davids presented a paper on "A new form of megohm resistance."

There was discussion upon this standard and others of the same type by Messrs. Pike, Wahl, Houston, C. Hering, and Billberg. (Referred for publication.)

Dr. Wm. H. Wahl gave "A *résumé* of recent developments in the manufacture of aluminium," in which he briefly traced the improvements of the past five or six years in both metallurgical and electro-chemical methods, and instituted a comparison between these two types of processes with the view of estimating the possibilities of each in the future cheapening of the cost of producing the metal. His remarks were illustrated by lantern slides of the principal methods of manufactures, and were discussed at some length.

There was also considerable discussion respecting the recommendation by a committee of the American association for the advancement of science of the word "alumin*um*" for the name of the metal, Prof. Houston speaking strongly in favor of the retention of the name "alumin*ium*," as being more in harmony with the nomenclature of the other metals, calcium, indium, magnesium, potassium, etc.

The meeting then adjourned.

L. F. RONDINELLA,
Secretary.

A NOTE ON SOME DANGERS IN ELECTRIC LIGHTING.

BY WM. MCDEVITT.
Inspector of the Philadelphia Board of Fire Underwriters.

[*Read at the stated meeting of the Electrical Section, held February 24, 1891.*]

After several years of experience in noting the dangers occurring in the use of electric lighting, it has been clearly demonstrated that in nearly every case the accidents resulted through ignorance both on the part of the workmen engaged in installing the wires and the lessee to whose care the new apparatus was entrusted.

These practical illustrations served as valuable lessons to those who witnessed them, more especially to the workmen, whose education in the business consisted merely in following plans laid out by the contractor.

Although the introduction of electric lighting has rapidly increased in our city, its use has been comparatively free from accidents, proving that most of the inherent dangers have been controlled, leaving but few features that require the attention of inventive minds to overcome. The most important of the existing defects is that relating to fusible connections, as the innumerable variety of alloys used for fuse metal is very misleading and is still an element of danger. Some of these compositions possess the properties of good electrical conductivity and are slow to heat; other compositions are of a character exhibiting weakness when heated, resulting in annoyance from continuous breaks and offering temptations (as has been found) to use ordinary wire in the absence of proper fuses.

Another universal danger in electric lighting results from the want of some uniform or more ready method of perfecting splices or joints in conductors. Some workmen are in the habit of making loose copper unions, leaving the solidity of the joint dependent on solder, which, being a metallic cement, is liable to be fused by a possible heavy short-circuit occurring on the line, thus melting the solder and leaving a loose connection.

Probably the most alarming danger exists through the possibility of lightning being conducted into houses lighted by electricity where the latter is supplied by aërial wires. The existence of this danger has been demonstrated where the house wires are attached to gas fixtures, offering a ready path for the lightning, which, in leaping from the charged wires to the gas pipes, carries the electric light current across, forming an arc, which pierces the pipe, and where gas is present, it will be ignited, causing a steady blaze. If this accident should occur near the ceiling, the building would be endangered.

With the rapid introduction of new appliances in electrical science, it may reasonably be expected that the defects above-mentioned will be overcome in the near future.

A NEW ACCUMULATOR PLATE.

By C. W. Pike,
Instructor in Electrical Engineering, University of Pennsylvania.

[*Read at the stated meeting of the Electrical Section, January 24, 1891.*]

While in Lowell, Mass., last year, a temporary craze upon the subject of storage batteries took almost complete possession of the town. The result of this was the production of several so-called new storage batteries. In reality, there was no attempt to produce a new type of accumulator, either by making use of new solutions or new metals. The sole novelty was in the form of the plates.

The inventors had but one object in view, namely, to construct a plate of such form that the active material would

not be dislodged from the cavities in which it was held, even by a severe shock or jar. Weight troubled them little; high discharge rate and large capacity less; and efficiency not at all. In spite of this, the results obtained by two of the inventors, Messrs. Bradbury and Stone, were not bad.

Two forms of plate were devised by these gentlemen, of which the first is very curious in its construction. Each plate consists of a number of elements, and each element is made by bending two strips of lead into what most of the

FIG. 1.

children, especially among the Germans, call a Jacob's ladder. (*Fig. 1.*)

This is then warmed and pulled in the direction of its length, taking care not to allow it to become twisted. When thus pulled out it has a number of interstices, into which active material can be placed. By placing several of these elements side by side and melting contiguous points together, a complete plate is formed, which is further strengthened by winding a lead strap around its perimeter, and melting points of the plate to it. On looking at the completed plate, it will be seen that every interstice on one side of the plate is connected to another on the opposite side of the plate, so that the active material, when put on, will consist of plugs of the shape seen in *Fig. 2*, so that the plug, as a whole, could not by any ordinary means be dislodged.

FIG. 2.

This form of plate has the advantage of large surface of active material and a fairly large ratio of weight of active material to weight of supporting plate, but the cost of making such a plate would more than overcome any advantages which it might have over ordinary forms of grid.

Realizing this, the inventors devised another form, made by bending lead strips over sticks of wood square in section, into the shape shown by *Fig.* 3. The first strip started from the top of the wooden stick, while the second strip started from the bottom of the stick; the third started from the top and

the fourth from the bottom, and so on. The plate, when completed, looked on either side like a checker-board: lead forming the black squares and the uncovered portions of the wooden strips the white squares. The strips were then

FIG. 3.

withdrawn and the plate strengthened, as in their first plate. This form of plate, though cheaper in construction than the former, was yet costly; so that they devised a mould, by which they were enabled to cast the plate entire. (*Fig. 4.*)

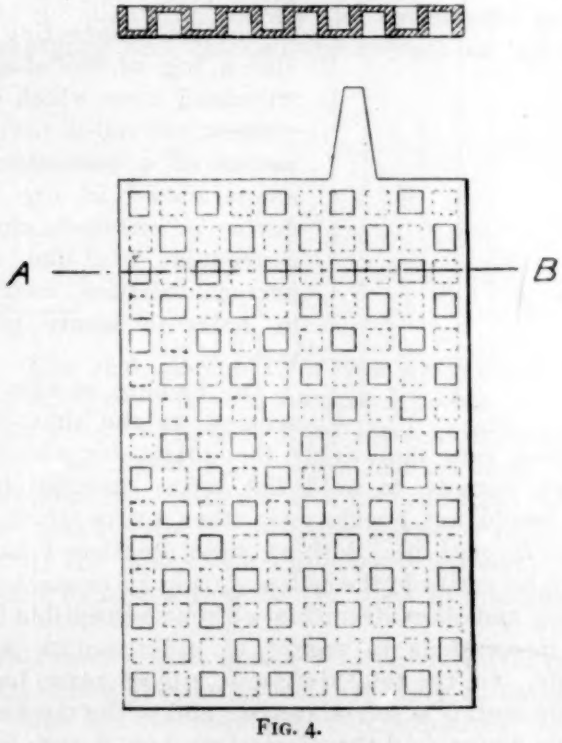

FIG. 4.

These plates before being filled are treated for a little while like a Planté, with a view to making the active material adhere strongly to the plate. The positives are

then filled with red lead, and the negatives with litharge. The active material can be put into the plates in three ways:

(1) As a paste.

(2) As a powder, forced in under pressure.

(3) By pressing the oxides into sticks and driving these in.

I have suggested another method, namely, casting the plates around these sticks, virtually using them for a part of the mould.

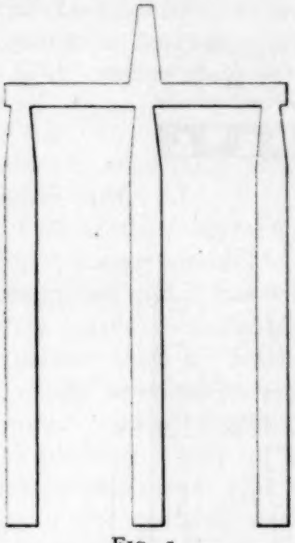

FIG. 5.

Each plate has a lug cast on its top, and the plates of similar polarity are connected together by a lead strip cast to these lugs. (*Fig. 5.*) On the top of this connecting strip is also a lug, of the shape of a truncated cone, which serves to connect one cell to the next by means of a connector, of the shape shown in *Fig. 6.* This device is extremely simple and convenient, but, like all connectors, requires watchfulness in order to secure good contact.

On looking at this plate it seemed to me that it would accomplish very thoroughly the object for which it was designed, namely, to hold the active material in. Also that it would not buckle even when a very strong current was sent through it. Both of these opinions I have since found to be verified; the cells enduring an immense amount of jarring and short-circuiting without perceptible injury.

My impressions in regard to other points were less favorable. Of the total weight of a filled plate, but thirty-eight per cent. is active material; and of the total surface of the plate exposed to the electrolyte, but thirty per cent. is active material. Further, the plates are so thick that the inner portion of the active material will not be acted upon when the cell is rapidly discharged, since the outer portion

which has changed from lead peroxide to lead sulphate prevents fresh sulphuric acid from permeating as far as the peroxide inside with sufficient rapidity to keep up the electro-motive force of the cell.

These considerations led me to believe that the internal resistance would be rather high, and its storage capacity and efficiency low, unless a low rate of charge and discharge was used.

Until recently the only tests made on this cell were by a gentleman in Sioux City, who obtained from cells, of which the dimensions were 5 x 8 x 8 inches, and the weight thirty-four pounds each, a storage capacity of 100 ampère-hours and a "quantity efficiency" of seventy-six per cent. with a charging and discharging current of about ten

FIG. 6.

ampères. The real efficiency, or the energy taken out divided by energy put in, was not stated by him.

The cell which I received from Messrs. Bradbury and Stone, was the *worst* which they had on hand, having been short-circuited for hours at a time, and having been allowed to stand with the solution partially evaporated. Consequently the plates were quite badly sulphated, and I did not have sufficient time to get them in first-class condition.

The instruments used were a Weston voltmeter reading to five volts and a Weston ammeter reading to twenty-five ampères. The test for resistance of the battery, made by the current and potential galvanometer method, gave, as was expected, a rather high result, varying from ·011 to ·016 ohms.

The next test was to ascertain whether it would give a current in excess of its normal current for any length of

time. It was found that it would give twenty-five ampères for, at least, one hour. The next test was to determine the capacity and efficiency under different conditions.

The method of testing was as follows:

Discharge the cell at the desired rate till the P. D. was 1·8 volts. Immediately charge at the desired rate reading volts and ampères until charged. Note the duration of charge, and compute the ampère-hours $A\,H$ and the watt-hours $W\,H$. As soon as charged, immediately discharge at the desired rate, reading as before; and compute ampère-hours $A'\,H'$, and watt-hours $W'\,H'$.

$$\frac{A'\,H'}{A\,H} = \text{quantity efficiency}$$

and

$$\frac{W'\,H'}{W\,H} = \text{real efficiency}$$

under the conditions of the test.

The result of three tests is as follows:

Average charging current.	Average discharging current.	Ampère-hours taken out.	Quantity efficiency, per cent.	Real efficiency, per cent.
12·07	15·	65	61	51
11·2	11·2	56	77	65
10·02	10.03	75	75	66

They show, as with any cell, the gain in using low rates of charge and discharge; but beside this they show that this cell is very inefficient at a rate above ten ampères.

After the first test the cell was left open-circuited for twelve hours, after which twelve ampère-hours were taken out before the P. D. reached 1·8. After a further rest of three hours, six ampère-hours were taken out. This recovery, found in all cells, as was expected, is very much exaggerated in this cell on account of the thickness of the plates, and the cells would, in an installation where the work was intermittent, give results considerably better than is indicated by the table above. In all probability the capacity, efficiency and rate of discharge would be improved by making the plates thinner.

In the following table I have made a comparison of the different makes of cell as regards storage capacity, weight and volume. Not having weight and volume of the plates themselves at hand, I have been obliged to use the weight and dimensions of the entire cell. It will be seen that as regards the volume per ampère-hour, it compares favorably with the others, but in respect of ampère-hours per pound it is deficient.

	Ampère-hours per pound of cell.	*Cubic inches per ampère-hour.*
Accumulator,	2·86	4·44
Bradbury-Stone,	1·85	4·44
Detroit,	2·65	4·63
E. P. S. Stationary,	1·91	7·06
E. P. S. Traction,	2·26	5·53
Julien,	2·81	4·33
Pumpelly,	3·00	4·85

It is probable that a cell with this plate better designed and in better condition, would give results much more comparable to the others as regards efficiency, capacity and discharge rate, while it would surpass many of them in durability, and I hope at some future time to present to the section the results of some tests upon such a plate.

A NEW FORM OF MEGOHM RESISTANCE.

By Richard W. Davids

[*Read at the meeting of the Electrical Section, held March 10, 1891.*]

The instrument of which this paper treats is not new entirely, but has been used for some years in England to a limited extent, so that the title refers mainly to its introduction in this country.

It is designed to take the place, at a small cost, of expensive and cumbersome wire resistances having approximately equal values, or the use of varying shunts, different electromotive forces and the wire resistances in ordinary use in measuring commercially the insulation resistance of cables, air lines, etc., by means of proportional deflections on a

sensitive galvanometer. The resistance of these boxes varies from one to five megohms, although these limits are not necessary, so that they may be had of resistances approximating to those which are met in every-day use.

So far as it is known to the writer the construction is as follows: On an etched glass plate of some 4 x 6 inches a pencil line is drawn resembling a sinusoid; experience teaching to a small degree, however, its breadth and length. The pencil used is probably an ordinary lead, or rather graphite pencil, of a hardness found empirically to give the best results; this line is burnt into the plate most likely with the use of a flux; this operation must be done slowly, and takes some time. After a rough test for resistance to find if it is within the range sought, it is mounted in a block on springs to take up jars and make connection with the ordinary tall binding posts of apparatus needing high insulation.

There is no attempt made at adjusting these boxes: the plates are made and measured, any that are too high or too low being rejected, the surfaces being ground off and made over again. In this way a comparatively cheap instrument is obtained. They are not laboratory instruments, but designed to meet the want of every-day line and cable testing, where speed and not great accuracy is desirable.

A somewhat similar instrument for laboratory work consists of a plate of unglazed porcelain, or even a sheet of paper, having pencil marks made on it; connection being made by means of springs resting on these marks. The objection found in these forms is that a breath of wind or jar may so change the contacts of the graphite particles as to seriously alter the resistance.

Some twenty of these boxes were brought to the writer for calibration, and the question arose as to the best way to measure them accurately and quickly. The first method tried was with a Thomson reflecting galvanometer, finding its constant with the one-ninety-ninth shunt, and sufficient resistance to give nearly the same deflection as with the meg and no shunt, using in both cases forty cells of chloride of silver battery.

As checks, other measurements were made using no shunts, and varying the electro-motive force; and also the bridge method. It is fortunate that there were two boxes of 100,000 ohms each at hand, as the second and third terms of the proportion, for with these calibrated in terms of a standard box which was the first term, it was possible to determine accurately the meg, the fourth term.

It was disappointing to find that the checks did not check, the figures obtained by the shunt method being much out. As the shunts were found to be practically correct, it could not be due to them, and on repeating the measurement with accumulators, the discrepancy disappeared, showing that the silver battery, even with small currents, polarizes. Everything seemed to go nicely now, perfect faith being reposed in the bridge method, when, in order to get a closer balance, the battery was considerably increased, with the disheartening result that it appeared that the resistance depended on the current used. This change was very small—less than one per cent.—and negligable in practice, but it seems to show that with such small currents as $\frac{1}{100000}$ of an ampère, appreciable change takes place, perhaps from heat, directly or indirectly.

It was an interesting point that the heat developed in such a box would raise one-sixteenth of a grain of water 1° C. in one minute. Supposing the resistance to be due to the imperfect contact of two adjacent molecules of carbon, the rise in temperature given could scarcely be enough to change the resistance.

It has been stated that change of temperature from 0° to 40° C. makes a difference of less than one per cent. in the value: also, that boxes made some years ago are practically the same now.

The use of the instrument was shown in one of the checks used. This was to find the comparative resistances of the various boxes by direct deflections, varying nothing but these with the megohms. With a very portable and accurate d'Arsonval galvanometer, the other boxes were checked rapidly; this was in effect measuring them, taking it for granted that any one was right.

In measurements on the street or in other places where it is difficult to set up and use an ordinary reflecting galvanometer, resistance boxes, different batteries, etc., all that would be necessary to get a very close measurement would be such a galvanometer, a suitable battery and such a megohm resistance.

BOOK NOTICES.

Lectures on the electro-magnet. By Silvanus P. Thompson, D.Sc., B.A., F.R.A.S. The W. J. Johnston Company, limited, New York. 1891. 280 pp. 75 illustrations, 10 tables. Price, $1.

No electrical book that has recently been published has been so welcomed and has been in such demand by electricians desiring practical directions for calculating and designing electro-magnets, as this most recent of the Johnston Company's publications.

Calculations concerning electro-magnets have been attended with much difficulty and considerable uncertainty until within recent years, owing to the lack of reliable data reduced to such shape that the average electrician can use it. Magnets for special purposes have usually been designed by means of repeated experiments and reconstructions; that course being preferable to very long, tedious and even uncertain calculation, which, in most cases, could not have been performed by the average designer of electro-magnets.

Researches in magnetism were deferred during the time when all attention was given to the production and application of electricity on a commercial scale, and to devising practical rules and methods of calculation. The lack of data concerning electro-magnets was severely felt, and it considerably hindered progress and the construction of efficient apparatus. Magnets for special purposes were usually guessed at and modified by proportion and experiment, and many were the dynamos and other apparatus using electro-magnets, whose failure or inefficiency was due to a lack of knowledge of the laws of magnetism.

A few years ago the current of investigation was, however, turned towards the principles of electro-magnets, in the desire to formulate the laws and furnish the much-needed data wherewith the work of magnets could be guided. Rowland's law of the magnetic circuit and Maxwell's law of traction were the bases of operation, and though much valuable information has appeared in the journals, no summary of a tolerably complete character had been published.

Prof. Thompson in his efforts to supply this great need, conducted a most thorough search through all the old and new English, French and German publications for records of reliable experiments and data about electro-magnets, and after collecting them and drawing his conclusions, added to them the result of numerous and original experiments of his own and pre-

sented them before the Society of Arts, London, in the form of four lectures, which constituted one of the sets of Cantor Lectures of the session, 1889–1890. These lectures were reprinted in the *Electrical World* during the past winter, and have now been issued in book-form, with some additions and revisions by the author.

With indefatigable labor, Prof. Thompson succeeded, by means of tables and simple formulæ, in reducing the calculations concerning electro-magnets to such practical shape, that the calculation of a magnet, to develop a definite number of lines of force, or to exert a pull of a definite number of pounds, is now a comparatively simple matter to a person possessing fair mathematical ability.

The first lecture contains, as is usual with Prof. Thompson, an "historical sketch" of electro-magnets, in which many of the famous magnets of the early days are described as well as many of the experiments made with them. The typical forms of magnets are briefly described next, and then comes a very clear and interesting discussion of the magnetic properties of iron and the methods of measuring permeability. A shop method of roughly estimating the permeability of iron by means of the "permeameter" devised by Prof. Thompson, is given and seems well suited to that purpose, as many of the objections to other methods are avoided. The lecture closes with a short and clear presentation of the phenomenon called "Hysteresis," and an interesting and instructive list of "Fallacies and facts about electro-magnets," many of which will astonish and surprise any one on first reading.

The second lecture is devoted chiefly to the general principles of design and construction. The principle of the magnetic circuit and the deduction of the formulæ is discussed in a very lucid way. The law of traction is developed and simple rules and formulæ with valuable tables and practical points are given, making the calculation of magnets for specific tractive force a comparatively simple matter. Then follows a discussion of the action of magnets at a distance from the armature, with and without pole pieces, and also the question of leakage and the reluctance of the air space between the poles; so that magnets for definite action at a distance can readily be approximated with considerable accuracy.

An appendix to the lecture contains a *résumé* of the calculations of the excitation and leakage of magnets, giving the rules and directions in a compact form.

The third lecture takes up the question of the winding of magnets. Many common-sense directions and suggestions are given and also a wire gauge and ampèrage table which will prove of great value to the designer of magnets. Unfortunately, however, it is calculated for the S. W. G. and not for the American gauge, and though the nearest B. & S. gauge is given, it is only a very rough approximation and will not do for accurate work. It is unfortunate that this table was not re-calculated and adapted to the B. & S. gauge throughout, as that would have made it far more valuable. A re-calculation of this table recently appeared in the *Electrical World*, but it is so full of mistakes that it is useless. The points given here about calculating the windings for magnets are very good and will prove very useful.

Next comes a discussion of special forms of electro-magnets; the effect of variation of the shape, size and position of coils, etc., are taken up and then the design of electro-magnets for special purposes, such as quickest action, longest range, etc.

Lecture four, is devoted to electro-magnetic mechanisms and gives the principles and practical directions for calculating magnets, whose cores or armatures are to have definite motions.

Altogether this little book contains a store of information of just the kind that is needed, and designers of magnets of all kinds will heartily welcome it, as one of the most useful and surely the cheapest of the electrical books ever published. H. S. H.

History of the American piano-forte: Its technical development and the trade. 12mo. By Daniel Spillane. New York: D. Spillane. 1890.

The author has succeeded in writing an interesting as well as instructive work. Beginning with a description of the spinet, harpsichord, virgina and other primitive keyed instruments, he proceeds to the invention of the piano-forte by Christofori.

All the early piano-forte makers of Europe and America are duly mentioned, and their instruments described.

Much time and trouble have evidently been taken by the writer to get the accurate dates, particularly of the early American patterns.

The high repute of the pianos of Messrs. Steinway, Decker, Steck, Chickering, Knabe, Miller, Weber, and others, are justly substantiated.

This section is followed by a description of the kindred branches—action-making, key-making and the manufacture of implements used in the construction of a piano—which is, of course, indispensable in a work on this subject.

The whole is supplemented by a short history of musical journalism in America, from the founding of the *American musical journal*, in 1835, up to the present day, so rich in art journals, the press having always been closely identified with the piano trade. In fact, the subject is exhaustively treated, and nothing of real importance has been omitted. The work is dedicated to the Franklin Institute, in recognition of the influence of its early exhibitions in stimulating invention in this branch of the arts. L. J. H.

Studies in statistics, social, political and medical. By George B. Longstaff, M.A., M.B., etc. Royal. 8vo, with maps and diagrams. London: Edward Stanford. 1891.

This large and attractive volume gives evidence of much care and expense. The data are chiefly from the general registry office, London, but also from other sources inaccessible to most of the public. Among the valuable studies are: birth, death and marriage rates in England and Wales for fifty years; growth of population of England; migrations of peoples; growth of new nations (the United States, Canada, South America, South Africa and Australia). All of these four and others in the work are illustrated by hand-

some diagrams. As there are sixty-one pages devoted to the growth of the United States, this alone should recommend the work to Americans.

A chapter on increased population of modern cities, of course, brings in many statistics respecting London and other cities, especially their food supply, etc. There are many valuable data on the death rates, rise, continuation and decline of various diseases, while the causes and relative prevalence of some of them are examined. One notable subject admirably treated, and having numerous tabulations, is the recent decline in the English death-rate, considered in connection with the causes of death. It is to be regretted, however, that this article, excellent as far as it extends, is not made up to 1890, inclusive, but only to 1884.

The volume contains thirty maps and diagrams (most of them colored), intended to aid the various subjects; and suggestions for the census and tabular data of various diseases are not the least among its excellencies. The author may well be congratulated on his successful application of a "hobby" to public education in statistical matters. Evidently he is an enthusiast on figures and data; but only such persons can compile works of value in this department. As the "studies" discuss subjects interesting to insurance men, physicians, attorneys-at-law and students of economics in general, it is to be hoped that the author will receive due reward for this effort.

N.

Triple-expansion engines and engine trials. By Prof. Osborne Reynolds. Edited by T. E. Idell, M.E. New York: D. Van Nostrand Company. 1890. pp. 191. 18mo.

This little book, which forms No. 99 of Van Nostrand's well-known "Science Series," is practically a reprint of Prof. Osborne's paper, read before the Institution of Civil Engineers, December 10, 1889, and published in vol. xcix of its proceedings, with an abstract of the discussion upon the paper.

It is exceedingly interesting, and places in a convenient form the methods and results of a series of observations which in themselves are well worth studying. The paper is a statement of the reasons for designing the engine of the Owens College laboratory in an unusual way, and contains a description of the engines and of numerous trials made with them. A short description of the engines may be interesting. The cylinders are 5 x 10, 8 x 10 and 12 x 15, and are arranged so that they can be worked as a triple-expansion condensing engine; as a compound condensing engine; or as a compound non-condensing engine; as a simple condensing, or as a simple non-condensing engine. Steam jackets can be used or not as desired. Large ports are provided and Meyer valves used, and the speed of the engines can be varied to 400 revolutions, and they can be worked with 200 pounds of steam. Each engine is arranged so that the work done by it can be absorbed in a Froude brake, ingeniously modified to fit the particular conditions. Each engine was allowed to run at the speed suited for using all the steam, and the cards were combined in such a way as to show the volume occupied by a given weight, say of one pound of steam in its passage through the cylinders. The latter half of the book is taken up with the discussion on

the paper, and about all the inaccuracies in the methods used are pointed out by Mr. E. A. Cowper, Prof. Unwin, Mr. W. W. Beaumont, Mr. Bodmer, Mr. Thorneycroft, Mr. Willans and others. The paper is well calculated to give the reader something to think about. H. W. S.

Sewage disposal works.—A guide to the construction of works for the prevention of the pollution by sewage of rivers and estuaries. By W. Santo Crimp. Philadelphia: J. B. Lippincott & Co. 1890.

The area of England is nearly the same as the area of the state of Georgia, while its population is nearly one-half that of the entire United States. Numerous commissions have been appointed, and numerous acts of parliament have been passed, for the purpose of preventing the contamination of the water supply of the cities of the kingdom by the sewage flowing from them. The volume before us presents the latest results obtained in the efforts made to improve the sanitary condition of this thickly populated country. Mr. Crimp's book is an octavo of over 250 pages, with numerous tabular statements, and is well illustrated by figures in the text, as well as by thirty-three lithographic plates drawn to scale. It is essentially a practical treatise, giving the actual working methods of sewage purification in thirty-three towns; a chapter being devoted to each town. Full particulars are given in each instance; such as the population of the given locality; daily flow of sewage; area of purification plant; methods of purification; cost of erecting plant; cost of working the process, as well as elaborate descriptions of the various methods of precipitating, settling and filtering the sewage. It may be of interest to state that of the various chemicals employed milk of lime, with or without the addition of a crude sulphate of alumina, seems to be the cheapest and most efficient, and is, therefore, most widely used.

This book should have a wide sale in this country. It treats of questions relating to public health, to which we shall soon have to give our careful attention. It will be particularly valuable to all sanitary engineers, and to municipal officials who have in their care the sanitation of our cities.

P.

Franklin Institute.

[*Proceedings of the stated meeting, held Wednesday, March 18, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, March 18, 1891.

JOS. M. WILSON, president, in the chair.

Present, 126 members and thirteen visitors.

Additions to membership since last report, twenty-two.

The actuary submitted the following extract from the minutes of the stated meeting of the board of managers, held March 11, 1891, viz:

"WHEREAS: The institute, at its stated meeting, held February 18, 1891, requested that the board of managers would express its opinion as to the advisability of the adoption of an amendment to article VI of the by-laws of the institute, offered by Mr. Wm. B. LeVan, viz: the addition thereto of a fourth section, to read as follows:

SECTION 4. No member shall be eligible for re-election to the office of manager, or be a candidate therefor, while he is filling such office, nor sooner than twelve months after the expiration of his term of office.

Therefore, Resolved, that the board of managers has considered the amendment proposed, and inasmuch as, of the thirty members of the board, twelve are nominated by the institute and elected annually, and as the amendment proposed would deprive the institute of the services of men who have faithfully served the institute for many years, without any corresponding advantage, the board would respectfully submit that in its opinion the adoption of the amendment would not be advisable."

Mr. William Kent, of New York, gave an oral description of the Springer torsion balance, illustrating his remarks by reference to a number of specimens of the apparatus, and with the aid of lantern views. (Referred for publication.)

Mr. John R. Jones read a paper describing his method and apparatus for rolling tapered axles. The paper was illustrated by means of lantern slides exhibiting the details of the machines for rolling the tapered forms and straightening the same after forming. (Referred for publication.)

Mr. S. Lloyd Wiegand described the Bacon machines for turning composite outlines in wood, to produce balusters, newel posts, and other ornamental forms for architectural uses, articles of furniture, etc. The work of these machines was shown by a large collection of specimens, exhibiting many complex and highly ornate forms. The machines are remarkable for the extraordinary rapidity with which they are able to operate, and for the smoothness of the work as it leaves the machine. A baluster or newel post of exceedingly complicated design is turned out in less than a minute.

Mr. Wiegand also described the Pentz boring and milling machines, and a universal joint used in conjunction therewith. This machine was recently reported upon by the Committee on Science and the Arts, and formed the subject of a recommendation of the John Scott legacy premium and medal.

Mr. J. Lynwood Garrison presented for inspection a suite of specimens of various articles fabricated from manganese-bronze, manufactured by B. H. Cramp & Co., of Philadelphia. These specimens illustrated, in an instructive way, the strength, malleability and other valuable qualities of this alloy. Mr. Garrison gave a short descriptive account of the history and various applications of this bronze.

The secretary's report embraced an account of the highly interesting and important discovery by Mr. M. Carey Lea, of Philadelphia, of certain allotropic modifications of metallic silver. A number of specimens were exhibited, which showed in a remarkable way the properties of the allotropic modifications of this metal, and their behavior towards various reagents.